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THE SCIENTIFIC MONTHLY

DECEMBER, 1920

AT TIMBER-LINE IN THE LAND OF TAHOSA[1]

AN INTRODUCTION TO THE AGED GNOMES OF AN ALPINE ELFIN-WOOD

By RAYMOND J. POOL

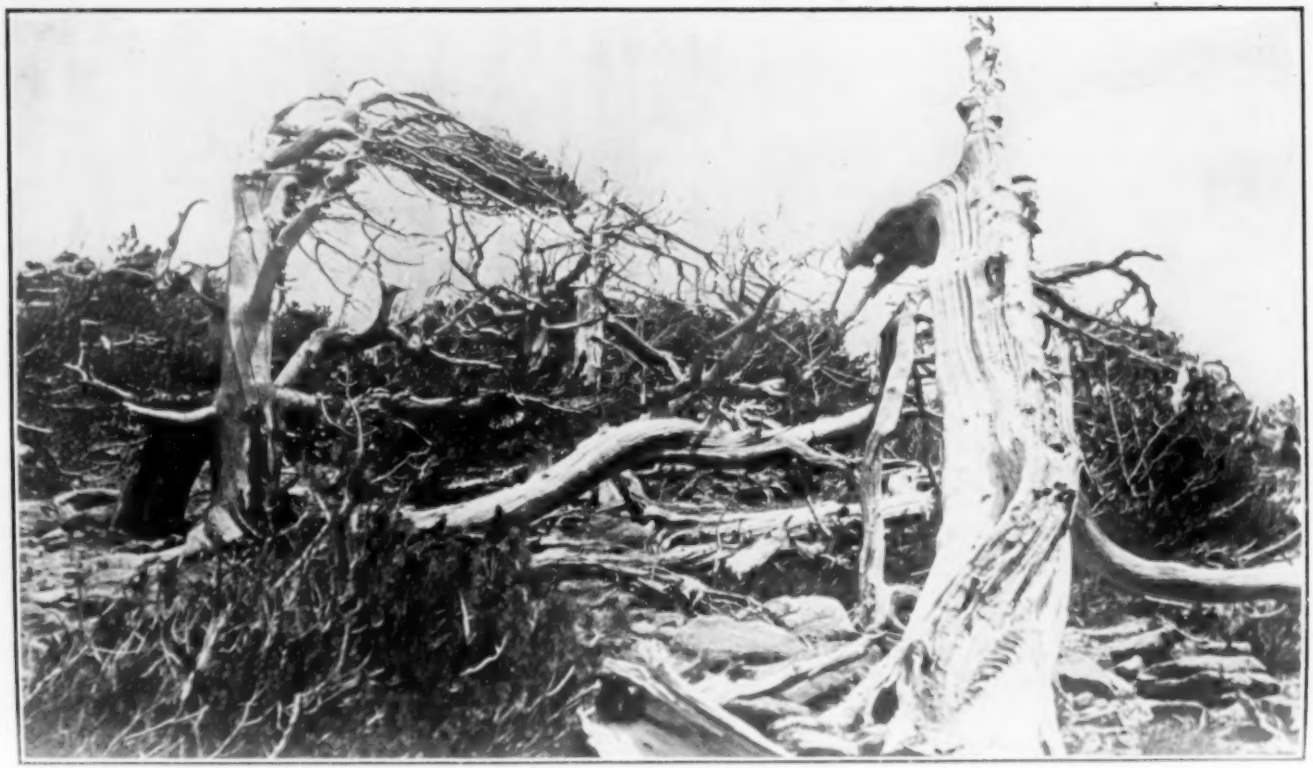
PROFESSOR OF BOTANY, THE UNIVERSITY OF NEBRASKA

FROM a land of beetling crags and tumbling cascades, from a land of snowy summits and booming storms, from a land of glacier-driven valleys and verdant forests, from a land of spire-crowned spruces and quivering aspen trees, from the land of Tahosa comes my story. Tahosa, the land whence myriads of sparkling mountain fonts bequeath perpetual laughter to the slender threads which, converging, evolve the mighty Colorado and start it on its silvery, winding way through the mighty chasm; a chasm sunken by the slowly laboring mechanic of the ages into the layered floor of that great plateau where the soft warm tones of the setting sun reflected from rugged cliff and treacherous talus mingle with the twilight haze of the great southwest.

THE LAND OF TAHOSA

Tahosa is the name that we would now use for one of our great mountain states had the request of those who petitioned for statehood been granted. "Dwellers of the mountain-tops," the name is particularly fitting for the state called Colorado, for that state claims a thousand mighty mountains which raise their rugged summits to altitudes exceeding two miles above the level of the far-off sea. There are in Colorado literally scores of mountain peaks which mount toward the sky beyond 13,000 feet, and many of these even exceed 14,000 feet. So Tahosa (if we may use the name) is indeed a place of mountain tops.

[1] Illustrations from Photographs by the Author.



THE STORY OF A TRAGIC PAST IS MIRRORED IN THE MANGLED FORMS OF THE STORM-TOSSED AND TIME-WORN TREES WHICH STAND FORTH AS THE OUTPOSTS OF THE ALPINE FOREST PRIMEVAL.

At Timber-line

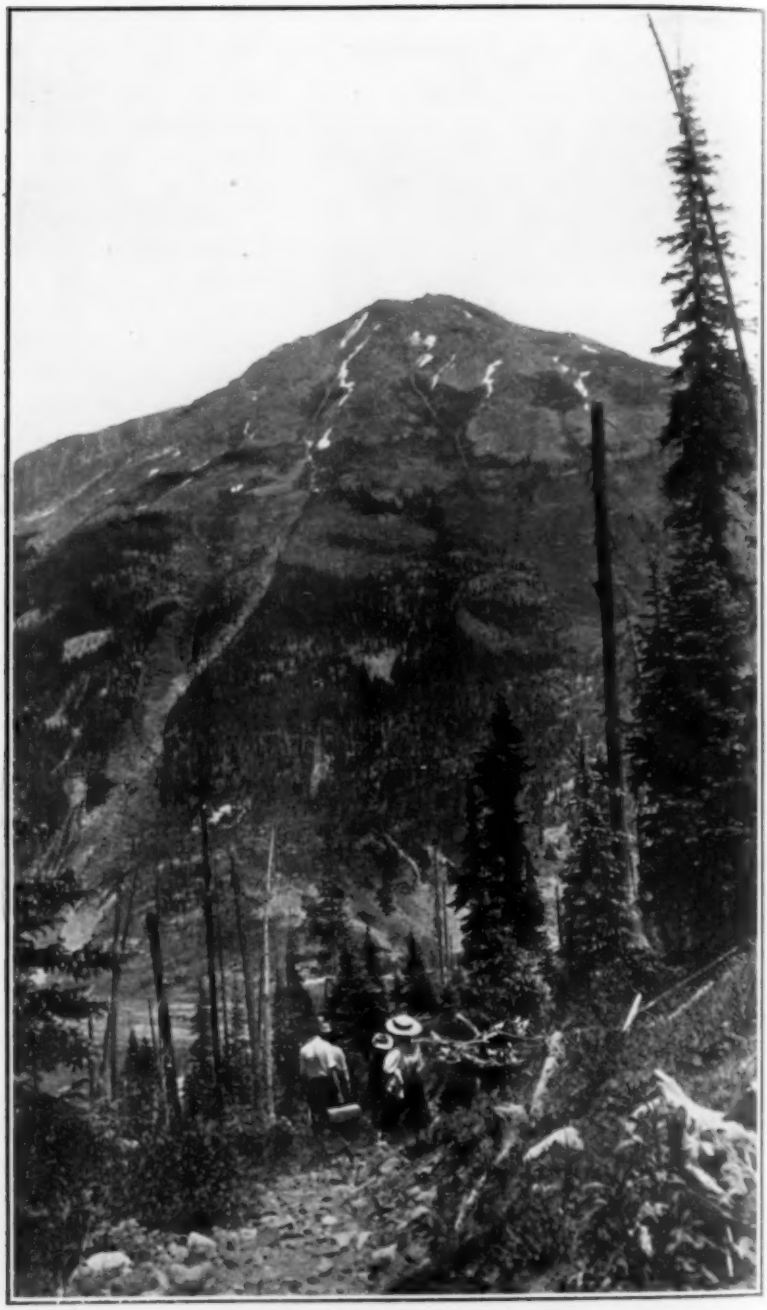
To one who loves the thrill of the dynamic landscapes of nature, and who delights in the muscular pull necessary "to go up to the sky in the mountains" there is no place at once so alluring and so thought-compelling as timber-line. The ever-changing forms and phenomena about one lure the climber to greater and greater heights. The rarefied atmosphere and an accelerated pulse draw and drive him upward.

At timber-line one beholds the upturned faces of the ancient past mirrored in the glacier-polished valleys and upon the lichen-covered cliffs of a granite cirque or in the jumbled, boulder-strewn moraine. He may read the story of a less ancient but none the less tragic past mirrored in the mangled forms of the storm-tossed and time-worn trees which stand forth as the alpine outposts of the forest primeval. The spirit of reminiscence blows full free upon the visage of those sad and tattered sentinels of that terrible frontier. One need only to possess the power to interpret the reactions of those outposts to catch fleeting glimpses of some of the most severe struggles and "last ditch" stands ever endured by living things. And I would impress my readers with the fact that these gnomes of the heights are living organisms! Many such sentries exposed to the scathing fire of a relentlessly opposing environment have, however, long since yielded up the ghost. Their whitened and eroded forms are being slowly ground away and their dust is gradually being returned to the barren earth which gave it.

The survivors at timber-line tell their several stories only to those who are acquainted with their lives and who delight in reading the dynamic life-histories and the evident struggles with environmental forces endured by poor dumb things, because the trees at timber-line, despite their expressive forms, are no more talkative than their close relatives who enjoy the equanimity of the mesophilous lowland forests.

So I will endeavor to portray in these few pages, and with the valued assistance of a few prized photographs, something of the biologic features characteristic of timber-line in the state of Colorado, and especially something about the peculiarly dwarfed and wrinkled and aged trees with which I have become more or less intimate.

The term timber-line is commonly applied to the upper limit of tree growth, whether that limit is an alpine or an arctic one. Beyond timber-line is the land of "little sticks," where there are no trees, but only miniature bushes, brilliant alpine flowers, mosses and lichens, and here and there enticing expanses of



TIMBER-LINE IS SELDOM A CLEARLY CUT AND STRAIGHT BOUNDARY LINE AT A UNIFORM ALTITUDE. Usually it is very irregular and jagged and it may swing up and down through an altitudinal amplitude of a thousand feet.

beautiful alpine pastures, the real *alps*. We must not understand that the upper border of the forest is in fact a clearly cut and straight boundary line at a uniform altitude or latitude as the case may be. Timber-line is commonly very irregular and jagged, and it may swing up and down through an altitudinal amplitude of a thousand feet or more, even upon a given mountain, or through several miles in the far north. Such fluctuations are related to numerous environmental obstacles some of which will be mentioned in the succeeding paragraphs.

The Trees at Timber-line

I have already stated that the most fantastic forms and perhaps, therefore, the most interesting phenomena of all in the most alluring medley of biological conditions at timber-line are seen in the dwarfed and wizened gnomes of the elfin-forest frontier. The commonest trees to reach the alpine forest limits in the Rocky Mountains of Colorado are limber pine (*Pinus flexilis*), and Engelmann spruce (*Picea Engelmanni*). Quaking aspen (*Populus tremuloides*), Alpine fir (*Abies lasiocarpa*) and fox-tail pine (*Pinus aristata*) also reach the front line trenches at timber-line, but are less frequently seen in such places.

The Engelmann spruce is seldom seen in the driest and most exposed sites. The species typically reaches its upper altitudinal limit in the form of scattered stands or isolated and more or less dwarfed individuals in the protection afforded by moist canyon heads. In such places one frequently finds a few poorly developed, erect, and excessively branched trees fringed at the base by an impenetrable thicket of scraggly and interwoven twigs produced by the multiplicity of lateral buds and branches. Such springy growths are excellent places upon which to enjoy a siesta, following a difficult climb.

It is quite different, however, for the limber pine which often braves the elements upon the most exposed ridges where the outposts endure the buffeting of the dynamic surroundings for years. This tree is able to maintain a foothold upon dry open tracts in the very teeth of the furious gales that often swoop down upon them from the heights.

Individuality in Trees

The individuality of the limber pines and the range in morphological reactions to the rigorous factors of their most extreme habitats are exceedingly varied and bizarre and are perhaps not to be equalled by any organism in any environment

Approaching timber-line with the Engelmann spruce. This tree is usually found in the moister habitats of the canyon heads at timber-line. It seldom occurs in the drier and more exposed sites with limber pine.

THE ENGELMANN SPRUCE IN ITS OPTIMUM SITE IS ONE OF THE FINEST OF AMERICAN CONIFEROUS EVERGREENS. It frequently reaches timber-line, but it is seldom found in the most severely exposed sites on that forest frontier.

MOST OF THE TREES WERE BENT TO THE LEEWARD AS THOUGH STANDING IN THE PATH OF SOME MIGHTY BEAST WHOSE TREAD HAD LITERALLY TRAMPED THE FOREST TO THE GROUND WHERE THE TREES WERE MORE OR LESS BURIED IN THE DÉBRIS OF THE AGES.

known to biologists. There are, in fact, scarcely two trees of this species to be found among the pioneers at timber-line that closely resemble each other in their bodily forms. Each tree appears to have worked out the solution of its own individual salvation under great stress and in its own individual manner. All this merely emphasizes the scarcely understood fact that plants do have considerable individuality even if they are not conscious beings.

The limber pine belongs to the white pine group, a series of species whose remarkable qualities and beautiful forms are famous among coniferous evergreens. This species is, however, a rather small tree, rarely attaining a height of sixty feet and a trunk diameter of four feet at the base. It is usually a slowly growing, stout, bristly tree from twenty to thirty-five feet in height, with a sturdy, rapidly tapering trunk from one to two feet in diameter at the ground level as it grows in the more congenial habitats of altitudes below timber-line.

Almost every conceivable modification and deformity of the limber pines occur upon the wind-swept alpine slopes and exposed, xerophilous ridges at timber-line. In a jagged area of elfin-wood at an altitude of about 11,000 feet containing several hundred individual trees of this species I was able to study and to photograph scores of morphological variations and eccentricities, the most of them peculiar to the timber-line neighborhood.

Sometimes an odd, squatty form of the normal tree is seen where the blasting effects of the gales are not directly felt. Such trees may be a scant dozen feet tall, but have developed, through the long decades of their precarious life, a rapidly-tapering and gnarled trunk three feet in diameter at the surface of the sterile rock heap in which the cramped roots find anchorage. These fore-shortened trees are often of great age.

Trees of Great Age

One can not judge of the age of these timber-line trees by their size or bulk. If one could evaluate their grizzliness in terms of years he would thereby be enabled to make a closer estimate as to their ages, but that is quite impossible. The layers of growth (the so-called annual rings) in the wood of such trees are commonly so close together or so thin as to be indiscernible to the naked eye. The sheeted layers of wood are often so attenuated that a good hand lens or a dissecting microscope is required to count them with accuracy. I found that some pieces of the wood of the timber-line limber pines which I

THE TREES LIE WITH THEIR COARSE ROOTS FIRMLY ANCHORED IN THE SOLID ROCK OF THE MOUNTAIN SIDE, WHILE THEIR HEAVY TRUNKS STRETCH AWAY TO THE LEEWARD LIKE THE UNDULATING BODIES OF PONDEROUS, PREHISTORIC SAURIANS.

brought home from one of my trips were of astonishingly slow growth. Some such pieces were tangential strips of wood, less than one inch in thickness, and by actual count it was found that they contained between sixty and seventy annual layers.

Mr. Enos A. Mills, a well-known writer upon the numerous natural features of the Rocky Mountains, states that there may be great differences in the ages of timber-line trees standing close together and that the size of a tree is no criterion whatever as to its age. He writes of a tree fourteen feet in height and sixteen inches in diameter which was three hundred and thirty-seven years old. Another tree close by was seven feet tall, five inches in diameter, and it had endured the trying life on that frontier for four hundred and ninety-two years! Doubtless many timber-line trees have maintained their uncertain foothold and have pieced out their lingering existence by the production of a thin new layer of sapwood during each brief alpine summer since the time when Columbus piloted his frail caravels across the heaving bosom of the unknown Atlantic.

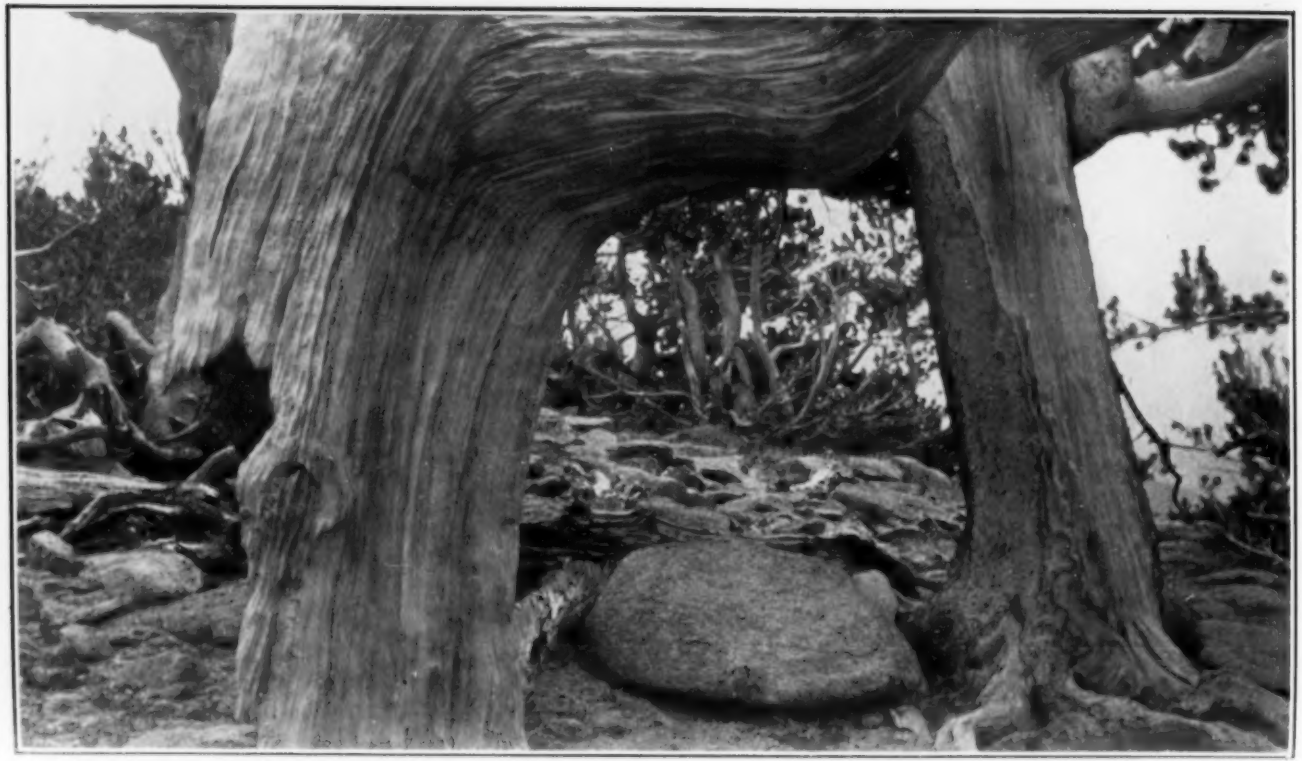
Wind-Crippled Trees

Besides the diminutive forms of the trees the most evident effects of the timber-line environment are undoubtedly seen in the wake of the winds which blow from a more or less constant quarter. Sweeping over some saddle or booming down the slopes the thundering blasts strike the sentinels of the forest and then sweep onward to the main body of trees farther down the slope. The immediate effect is to produce odd, lop-sided creatures more or less similar to those seen upon windy sea coasts. The crowns of the trees become distorted and bent to the leeward and often the whole tree is thrown out of the perpendicular, with a resultant deformity more striking than the sea-side cripples. The branches on the lee side stretch far out as if in flight before the oncoming blasts. The lateral twigs and the normally erect spray become pounded and woven into most astonishingly abnormal brushes and bristling tufts. The one-sidedness of such trees is frequently still further emphasized because of the failure of the branches to develop on the windward side or by the cutting away of any such branches by the incessant beating of the prevailing wind. I have examined some such trees in which there were absolutely no external indications of there ever having been any lateral branches on the windward side of the trunk.

The extreme effect of the wind combined with heavy snowfall is seen in the development of prostrate trees lying upon

SUCH AGED AND SQUATTY TREES MAY BE A SCANT DOZEN FEET TALL, BUT THEY HAVE DEVELOPED, THROUGH THE LONG DECADES OF THEIR PRECARIOUS EXISTENCE, A RAPIDLY TAPERING AND GNARLED TRUNK THREE FEET IN DIAMETER AT THE BASE. They may be more than five hundred years old.

THE INCESSANT CHISELING OF THE WIND FINALLY ENDS THE LIFE OF THE DEFORMED STRUGGLER AND THEN ITS LIFELESS BODY STANDS OUTLINED AGAINST THE HORIZON LIKE THE EMACIATED FORM OF A CRIPPLED GIRAFFE GROWN GAUNT FROM LACK OF FORAGE AND DRINK.



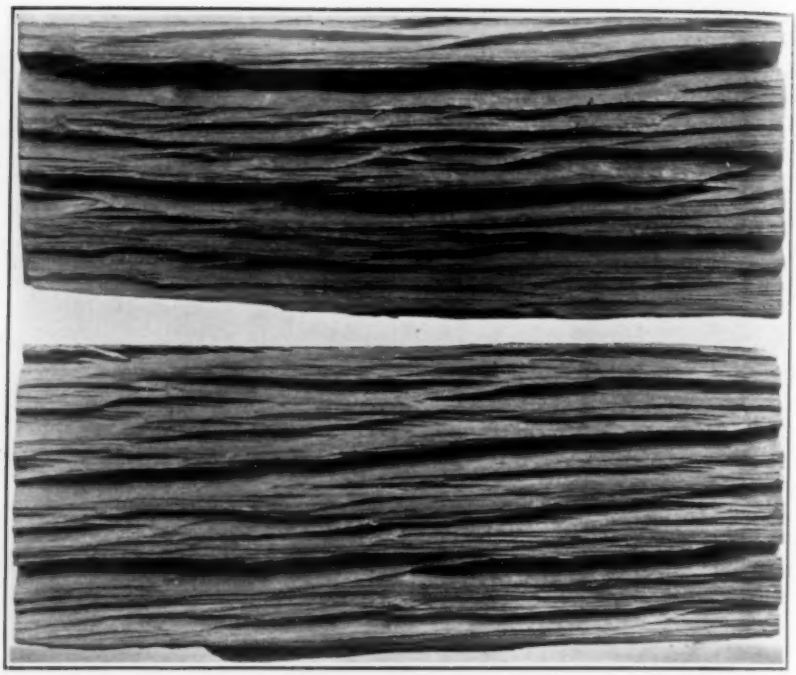
After the bark has been pulverized and weathered away the teeth of the wind continue to etch their way into the very wood itself.

the stony surface of the wind-blown mountain floor. These trees lie with their coarse roots anchored firmly in the solid rock of the mountain side, while the heavy trunks stretch away to the leeward like the undulatory bodies of ponderous prehistoric saurians. One such tree eighteen inches in diameter and twenty-five feet long had had all of the branches on the upper surface planed away by the abrasive sand and sleet carried by the wind, while the living twigs were segregated in the usual tangles toward the end of the prone form. The flattened forest at that point as portrayed by the above trees illustrates the unmistakable influence of the heavy snowfall at high altitudes. None of the trees in that prostrate group raised a twig higher than five feet above the ground. The most of them were flattened out and bent to the leeward as though standing in the path of some mighty beast moving with the breeze and whose tread had literally tramped the forest to the ground, where the trees were more or less buried in the débris of the ages. In less exposed situations the trees assume a more nearly erect posture, but even there the knotted, twisted and burly forms are sometimes riven by lightning and disheveled by the blasts of Boreas and his heavy mantle of snow until they totter like a drunken man and almost fall to the earth.

The dwarfed, gnarled, twisted and sprawling trees at timber-line have been known to botanists by the term *Krummholz*, which is German for *gnarled wood*. The term was originally applied to the high altitude form of the mountain pine (*Pinus montana*) of the Alps. In late years it has been used widely as a general term for the scrubby and bizarre growth of woody plants at or in the vicinity of timber-line on mountains in any part of the world. It is surely a very expressive and useful term.

As one strolls among the various forms at timber-line he occasionally finds a curious entanglement of scraggly, woody growth lying in the shelter of a low boulder or snuggling beneath the projecting angle of a cliff. In such places the branches and leafy tufts are trimmed smoothly away on a level with the surface of the protecting obstacle. The twigs of such trees often become so densely interwoven and so tough that one may walk over the prostrate, hedge-like form without breaking far into the brush beneath the surface.

The degree of exposure to the wind and the influence of heavy snow appear to be most effective in modifying the forms of the trees at timber-line. The effects of snow are also seen far below the upper limit of tree growth, particularly in



So deftly are these wind-carvings executed by the fingers of the wind that they defy the best efforts of a skilled wood carver who would attempt to imitate them. The above pieces show the effect of wind erosion in comparatively straight grained wood.

thickets of quaking aspen trees. There one occasionally notices that the trees are bent and twisted and crooked into multifarious contortions at a certain height. I once saw such a striking condition in a grove of young aspens about twenty feet tall. The distorted portions of the trunks were completely confined to a zone between five and seven feet above the ground. And every tree of the several hundred in the grove was affected. Very evidently the cause of such a peculiar-looking condition was to be found in snow relations, coupled with the growth habits of the trees.

Wind, the Wood Carver at Timber-line

An additional point of great interest in connection with my studies of the *Krummholz* at timber-line in Tahosa are the fantastic wind-carvings produced in the wood of the decorticated tree trunks. The winds at timber-line often blow with a high velocity and then they carry with them quantities of sharply angular rock fragments weathered away from the very back-

bone of the mountains. During the long winter months this load of abrasive material is augmented by the presence of quantities of snow and ice spicules. The fine mineral particles have been so recently removed from the parent rocks that they are exceedingly sharp-angled and pointed. This feature increases their abrasive influence as they are carried along by the wind. A winter gale thus laden, sweeping over the heights, may in truth be said to possess teeth, the "teeth of the storm."

I have already pointed out the fact that the windward sides of the tree trunks at timber-line are sometimes devoid of branches. Frequently the bark also has been cut away from that side. The tree may be nearly girdled by the powerful grinding effect of the sand and sleet carried in the high wind. Often only a slender longitudinal belt of living bark is left on the lee side of the tree to carry the necessary life-giving substances from the meagerly developed expanse of foliage to the hard-working root system pinched and cramped in the crevices of the rocky substratum. The breakage of this connection is doubtless an important factor in the final death of the trees which grow in the most exposed sites. The incessant hammering and chiseling of the wind ultimately ends the lingering life of the deformed struggler and then its lifeless body stands there outlined against the horizon like the emaciated form of a crippled giraffe grown gaunt from lack of forage and drink.

After the bark has been pulverized and weathered away the teeth of the wind continue to etch their way into the wood itself. The thin, young and less resistant sapwood is soon removed and then the harder, resinous heartwood is attacked, upon the surface of which are cut the intricate traceries of the fingers of the wind. So deftly are these carvings executed that they defy the best efforts of the most skillful wood carver who would attempt to reproduce them. Their imitation is impossible because the fingers of the wind have felt out the variations in hardness and structure of the wood with a delicacy of perception far beyond that possessed by a mere human being. The result is the production of wood carvings of remarkable beauty and variety. The unevenly resistant wood and the intricate variations in the grain of the slowly and irregularly growing wood are strikingly mirrored in the variation of detail in the individual trees. The most beautiful effects of all such wind influences are seen in those tree trunks and branches in which the grain is more or less distorted. Thus the spiral growth, wavy grain effects, and the extreme disturbances of normal wood structure in knots and burls, coupled with variations in wood

density, are emphasized in a very remarkable and often beautiful degree upon the dead surfaces of the storm-tossed trees at timber-line. The erosion of the woody bodies continues after death and serves still further to multiply the fantastic and weird effects of the environment in which the *Krummholz* is developed. Even the fragments of the dismembered bodies of the ancient gnomes are blown about and sculptured until they lose nearly all semblance to any plant structure. Waugh's uniquely illustrated and more or less spooky fairy tale, "The Clan of Munes," might find a natural sequel among these grotesque forms at timber-line.

Timber-line Contrasts

It is only with great difficulty and careful study that the occasional visitor is able to acquire an adequate appreciation of the peculiarly rigorous environmental conditions at timber-line. This is particularly true for him whose visits to timber-line are made only during the relative peace and quiet of a warm summer day when the alpine meadows and fell-fields are ablaze with the blooms of gorgeous wild flowers and when all living nature on the heights is humming with the busy opportunity for life and propagation afforded by the short hours of a brief growing season. The first blush of vernal bloom has scarcely faded upon the alpine heights ere the seeded capsules of arctic willow and primrose and the hoary plumes of Dryas give warning of the early approach of old Boreas with his host of forces to try the last fiber of every living thing at timber-line throughout the long, cold winter. No more striking contrast can be found in nature than that seen in the silent places where brilliant wild flowers bloom in lavish profusion within the shadow of frowning cliff and the storm-marked and ghostly *Krummholz*, where the youthful brook gurgles along its silvery path hard by where the trustful Ptarmigan silently guards her young, and where the warm sunshine and cloud-bedecked, summer sky covers all. John Muir's words are expressive in this connection: "Nature's sources never fail. Like a generous host, she offers her brimming cups in endless variety, served in a grand hall, the sky its ceiling, the mountains its walls, decorated with glorious paintings and enlivened with bands of music ever playing."

Besides the limber pine and the Engelmann spruce which enter most prominently into this sketch there are several other species of coniferous trees which reach timber-line in other por-

tions of the continent. White spruce (*Picea canadensis*) and balsam fir (*Abies balsamea*) in the New England mountains; alpine fir (*Abies lasiocarpa*), white-bark pine (*Pinus albicaulis*), and fox-tail pine (*Pinus aristata*) in the Rocky Mountains; and the mountain hemlock (*Tsuga mertensiana*) from Oregon to Alaska are other trees which share in the development of timber-line phenomena.

The altitude of the alpine timber-line varies more or less inversely with latitude, *i.e.*, the upper limit of tree growth on mountains is encountered at a lower altitude as one travels northward, as latitude increases the altitude of timber-line decreases. In the far north timber-line comes down to the shores of the icy sea shortly beyond the Arctic circle. The altitude of timber-line in the Canadian Rockies varies from 5,000 to 6,000 feet. On Mt. Rainier it is about 7,000 feet, while in central Colorado it is reached between 10,000 and 12,000 feet. The highest timber-line in the world is said to occur in Mexico, as on Mt. Orizaba, at an altitude of 14,000 feet, which is reached by *Pinus strobiformis*, one of the southwestern white pines.

The Causes of Timber-line Reactions

The limitation of tree growth at certain altitudes in the mountains has never been adequately explained. Unquestionably the cause or causes are to be found somewhere in the maze of interacting or overlapping factors brought to bear upon the life at timber-line. The ecological relations there are apparently very complex and until much more quantitative research has been completed within those dynamic habitats biologists will be unable to point out the cause of timber-line with surety. The effects of wind are unmistakably chiseled into the prominent reactions of the trees, some of which have been poorly and inadequately described in the preceding paragraphs. The effect of heavy and long-persisting beds of snow are probably of importance in restricting the growth of forests at high altitudes. Certain investigators have felt that snow is the chief factor which inhibits the development of a forest of normal trees at certain altitudes and is responsible for the development of alpine grasslands instead of forest. Others believe that the desiccating influence of the winds, coupled with a deficiency of available water supply at the roots and a low temperature, largely explain the stunted nature of the woody growth at timber-line. Atmospheric rarity, intense insolation, rapid radiation, low relative humidity, and short growing seasons have

also been proposed as playing important rôles in the production of alpine dwarfs and possibly in actually causing timber-line.

Experiments appear to have demonstrated that the dwarfing is actually due to the alpine conditions. Portions of alpine plants moved to the lowlands promptly awakened from their dwarfed state and developed the characteristic lowland forms with tall stems, while portions of lowland plants established in alpine sites promptly became dwarfs.

Snow-cripples, drought-cripples and dwarfs as well as wind-cripples and dwarfs probably occur within timber-line areas. Numerous similarities existing between alpine and polar growth forms and the vegetation of desert lands have led to the belief that the whole relation is one of aridity. Arctic-alpine habitats have been included among the desert series in ecological classifications of habitats.

The prevailingly low temperature summation is doubtless of great importance in limiting the activities of the plants at timber-line and consequently in modifying the growth forms most efficient or possible there. In this connection I once recorded some observations upon the matted forms of the alpine pink (*Silene acaulis*) growing in a gravelly slope on Mt. Garfield in the vicinity of Pikes Peak. The flat, bristly, mat-like plant, beautifully radiate in form, was about eighteen inches in diameter and was growing where it was partially shaded by a large boulder. The shaded portion of the mat was evidently not in as good condition as the portion which received the direct rays of the sun, in fact it possessed many of the well-known weakly characteristics of shade plants in general. But the influence of the sun (heat or light) was very evident the day I made the observations because of the production of flowers on the plant. The arctic pink at blooming time is often literally covered with scores of the beautiful pink flowers about five-sixteenths of an inch wide. The lighted portion of the mat in this case, the portion receiving the direct rays of the sun at noon-day, was about one half of the whole plant and was blooming profusely, while the portion in the cool shade of the rock showed very few open flowers or flower buds.

A primary task of all green plants in timber-line situations is certainly to utilize the heat and light of the sun in the manufacture of the food supply necessary for leaf and root formation and for reproduction if they are to remain in such places. Essential nutritive materials must be secured *via* the root-system from the cold soil solution at the same time. Any condition of

air or soil which tends to block these fundamental processes for long periods of each year strikes at the very life of the plants in question, and consequently would exert a significant influence upon the initial establishment and the subsequent success of the plants on the heights.

These are but a few of the impressions and problems which are suggested on every side to the inquisitive wanderer among the plant pigmies at timber-line in the land of Tahosa.

THE SOCIAL NEED FOR SCIENTIFIC PSYCHOLOGY[1]

By Professor KNIGHT DUNLAP

THE JOHNS HOPKINS UNIVERSITY

IT is a matter of common report that never before has there been in the United States such an interest in psychology as is now evidenced in manifold ways. There is an increased enrollment in psychology classes in colleges and universities. There is an increased sale of books bearing upon various phases of psychology, and a heightened activity in the writing of such books. There are demands coming from the commercial and industrial fields, for experts in psychology, and for psychological methods. There is an awakening of interest in psychology in the ranks of those who have to deal with social and economic problems, and there are signs that among the members of the medical profession the need for psychology will soon be recognized.

There is an especially significant demand for applied psychology; not only for the applications of mental tests, but also for the application of the laboratory research methods to problems of industry, of education, and of hygiene. The resources of my own laboratory, for example, have for the past year been very largely devoted to work laid upon us by the American Committee for the Study of the Tobacco Problem. It is noteworthy also, that large industrial and commercial corporations are now seeking for experimental psychologists to conduct research for them along specific lines of scientific need.

The increase of interest in psychology is being shown also in the great vogue of the mental testing of school children, and in the application of mental tests to applicants for entrance to college, as well as in the concomitant rapid increase in the types and forms of single and composite tests.

The interest which is apparently active, is unfortunately not shown in the way on which psychologists themselves would lay the most stress, as indicating a substantial, rather than ephemeral interest: namely, in the increased provisions for the adequate training of psychologists, and the increased provisions

[1] Presidential address before the Southern Society for Philosophy and Psychology, New Orleans, April 23, 1920.

for the effective working of psychologists along the lines of pure science. Perhaps this provision will come as a natural result of the wave of popular interest: perhaps it will not come unless definite measures are taken to bring it about. This uncertainty must cause us to pause in our self-congratulations, to consider whether, after all, psychology is experiencing a healthy growth, or a process of inflation, or even a sort of parasitic growth which may sap its strength and lead ultimately to its downfall and disrepute.

We must admit that the large growth of interest of which I have just spoken, is a growth in interests in various sorts of things, which happen to be included or held together by the name "Psychology," but whose actual affiliations with each other require careful determination before we can accept the mass as one subject. Some of the lines of growth of so-called psychological interests are destructive of other lines; and while it is true superficially that there is this great increase in what is called "psychology," it may not be true that this interest coincides with that which many of us hold to be the essential spirit of psychology.

It is a matter of common knowledge to all, that not every one who labels himself a "psychologist," or to whom the label is tossed by journalist and popular writer, is entitled to the designation. We are frequently reading, or being told, that "psychologists now admit thus and so": or, "an eminent psychologist claims such and such": and then, on further inquiry, finding that the eminent psychologists referred to are Professor Oscar Loon, Caroline Greenfield, and W. H. Spinks, M.D. We find also an appalling number of self-annointed "psychologists" in the industrial field, industriously selling psychological gold bricks to the gullible business man, and raising thereby obstacles to real industrial psychology which will long endure.

It is sometimes difficult to explain to the layman the difference between the real psychologist and the alleged psychologist. We can, however, usefully point out a fairly accurate basis of discrimination based on the endorsement of the American Psychological Association, and deny that any one not a member of that Association can be recognized as an "eminent" American psychologist unless specifically endorsed in some other way by that association. The conditions attending the present wide interest in psychology and pseudo-psychology make it imperative that we guard membership in the American Association more carefully in the future than we have in the past, and admit to official recognition no one who may use his endorsement to the detriment of science.

The assumption by unauthorized parties of the function of the psychologist is not, however, the only symptom of the seriousness of the situation in which psychology finds itself at the present time, nor is it the most serious symptom. We might expect to find a common interest in psychology and a common purpose in respect to it, among those who not only are endorsed by the official company of psychologists, but are still further accredited by specific appointment to positions in psychology in our great institutions of learning. But here we find so great a diversity that it would seem that an increase in interest in what some of these men call by the name of psychology is a withdrawal of interest from what some of the others call by that venerable name. We find in the list of accredited psychologists, for example, those reactionaries who would have no advance beyond the conceptions of John Locke and Wilhelm Wundt, and also those radicals who would altogether abandon psychology as it is historically known, and would admit the possibility of no biological science beyond physiology.

Our diagnosis of the weakness of the present psychological situation can not stop even here. We find among us those who, regardless of their being otherwise reactionaries or radicals, or even moderate conservatives, are weakening in their devotion to science; and to whom her hard discipline is becoming so irksome that they are favorably disposed to that mysticism which is ever the enemy outside our gates. These faint-hearted ones have sounded the praises of the alien goddess, and have proposed the surrender of the citadel to her forces. This danger may not be so serious as it seemed two or three years ago, but it is not yet absent.

Such is the situation. And in the face of this, the need in society for a real and constructive psychology is becoming daily more specific and more urgent. This need we seem to be supplying only in a small measure, and I believe we can supply it with any degree of adequacy only by means of a greater emphasis on the scientific aspect of psychology.

To make my meaning clear, I must insist upon the distinction between the various arts which derive their support and extension from psychology, and psychology itself. I shall insist in the second place upon the distinction between psychology in the general or inclusive sense, and that particular sort of psychology to which I am here applying the term *Scientific*. In order to further my exposition I shall also assume two postulates which I do not care to defend at the moment, but which I think no one would wish to dispute.

The first postulate is: That a science which makes no appreciable progress in its conceptions and methods is dead. The other is: That a proposition to sweep aside the history of a science and start completely anew is suicidal; that it is an admission that the newly proposed scientific foundation is as insecure in value as the rejected foundation is declared to be.

Of the value of the psychological arts there can be no doubt. I do not wish to be understood as in the least hostile to the arts, for such would be a complete misunderstanding of my meaning. On the contrary, I am profoundly interested in the expansion of these arts, and am personally engaged in the furthering of several of them. Nevertheless, my interest in the present discussion is in the science which underlies these arts, and which has values not only in its fostering these arts, and as pure science, but also in direct relations to society which are entirely independent of its functions as exercised indirectly through the psychological arts.

It has been said by some of those who were concerned in the application of the well known Army Mental Tests, that there was no psychology involved in either the construction or the application of these. Some of those here present have heard this statement emphatically made. Men who themselves are presumably well grounded in psychology, and who have held academic positions in psychology, have averred that in the army work they neither used nor needed anything that they had learned in any course in psychology or in a psychological laboratory.

I must say that, with all due respects to those who make this claim, I do not think they are right in their analysis. If this were so, it would indeed be a strange thing that the work of psychologists was required for the construction of the tests, and for the organization of their application: and we should have to look for some reasonable explanation for the fact that this work waited on the efforts of the psychologists. But nevertheless, underlying this claim there is a fact which we must give consideration. We must admit that after such tests are compiled and organized, the giving of such objective tests is merely a matter of technique, requiring a special clerical training, which may be acquired by any one of the requisite native intelligence and general education, without any orientation in the matters which are generally contained in lecture and laboratory courses in psychology. It is also true that a considerable amount of success has been obtained in the selecting, if not in the construction, of such tests, by persons wholly devoid

of psychological training: in fact, in the field of commercial applications by men whose psychological training has a negative value.

It is true, moreover, that the greatest source of the evil which flows from the widespread "mental testing" carried on in schools by enthusiastic teachers, and others, is not the ignorance of general psychology which these "wild" testers show, but their lack of training in the technique of giving and interpreting the tests. The successful giver of tests need know nothing of the results of research on memory, or space perception, or any other department of psychology. It makes no difference for his work whether he assumes that the soul is a real spiritual essence, or a material substance which is visible after death as a ghost, or that there is no soul. He may believe consciousness to be an act, a stuff, or a myth. He may be a Christian Scientist, a Theosophist, or a Hegelian. He may know nothing of the niceties or the difficulties of threshold determination.

It is true that the fundamental parts of the technique required are inculcated in psychological laboratories. But so is, in far greater measure, the technique which the oculist requires (and usually does not possess). This would be a relevant matter only if we should find out that, after all, psychology is nothing but technique—a conclusion which I certainly shall not admit as yet.

What we are bringing out clearly here is, that the whole subject of mental testing is an *art*, and that like any art, it may go a great way without any scientific foundation. But this is not the same as the admission that there *is* no scientific foundation, or that the art can without detriment to itself, dispense with the foundation. It is true that we have at least one example of an art which has reached a high stage of development with practically no scientific foundation at all. This is the art of music, which is a most finished and practical art, although there is almost nothing in the way of a scientific basis for it. But we need not stop to point out that the conditions under which this art has arisen are sharply different from those surrounding other psychological arts.

Other psychological arts which might be mentioned along with what for convenience we call "mental testing" are: hypnosis, character analysis, and education in the restricted meaning of the term. The first two of these have languished for lack of a psychological scientific foundation which I believe will eventually be supplied, and the third is the grand example of the value of that foundation.

It is not necessary for my purpose to go further into details on this point, but rather is it expedient to consider the nature of the science which I have assumed as underlying the arts.

The two great and inexorable conditions laid upon every science are that it shall in the first place be *empirical*, and in the second place that it shall be *logical*. Without an empirical basis of fact, even with the utmost logical consistency, we can evolve nothing more than an ingenious speculative system. Without logical consistency in reasoning, the attempt to explain facts results in nothing more than mythology. These conditions are laid upon psychology no less than upon physics and chemistry, and the chief hindrances of psychology in the past have been the results of the difficulty in approximating to both of these ideals. At the present time, the movements which threaten to disrupt or destroy psychology can be analyzed into omissions of scrupulous regard for one or the other of these great principles.

Many attempts have been made to construct psychological systems on a defective empirical basis. Such a system was constructed with brilliant regard for consistency by William James, and called by him the "stream of consciousness." It was finally discarded by him because he subsequently recognized that he had omitted the empirical basis, and so had elaborated a theory of the "mind" defective only in one respect, but in the vital respect, that there was no evidence that any such mind ever existed.

In a still more thoroughgoing way, Malebranche constructed the system so beautifully presented in his *Dialogs:* the system which so impressed John Locke and George Berkeley that they expanded it, and passed it on through Hume and Kant into the Anglo-German psychology which still clings to it, in spite of the unmistakable lack of an empirical basis.

The most recent construction of this *a priori* sort is the psychology which calls itself Behaviorism, which reaches a conclusion apparently quite different from that of the reactionary psychology, but by the same method. And of the making of more systems there is no end, so long as we start from concepts instead of facts.

The effects of the neglect of the other principles of science (logical consistency) are perhaps most strikingly illustrated in the system or group of systems which are variously known as "Psycho-analysis," "Freudianism," or "The newer Psychology." While the fundamental defect of this system is its lack

of an empirical basis, it reaches its most astonishing results by the complacent ignoring of the elementary principles of deductive and inductive reasoning. But psycho-analysis is by no means the only sinner in this respect, even if it be the most flagrant one. So long as we maintain our calm indifference to the meanings of the terms we use, preferring to keep the meanings vague so that we can juggle them as we need to make them serve our purposes, psychology is certain to be prevented from reaching its place as one of the sciences.

There is a consensus of opinion historically as to the data with which psychology ought to start. Aristotle, whom we rightly regard as the founder of the science, mapped its empirical basis with fair accuracy. The psychologists who followed him, even Malebranche and his disciples, were really seeking to start from the same facts. We are by no means justified in discarding the efforts of these men, but rather we owe it to them, as to their heirs, to better their efforts and avoid their failures, as we trust that our successors will better our efforts and avoid our failures.

The situation in which psychologizing begins, and hence, from which the science of psychology starts, is *the being aware of objects:* and unless psychology starts from this it does not start at all. This is the history of the science, and this is the conclusion to which any one must come who searches to-day for an empirical basis on which to build a science which will not be merely a misnamed part of some other science. On the other hand, this is a real empirical basis, since *being aware of things* is an undeniable fact, however we may ultimately explain or analyze it.

This empirical basis at once excludes behaviorism, which may be a perfectly legitimate part of physiology, but as an attempt at psychology it is ruled out as non-empirical, since it does not stand on the basis of the complete empirical situation.

Further examination of the *facts of experience* (as we customarily name the empirical basis of psychology) reveals three facts, which are revealed *as facts*, and not as inferences. First, that awareness occurs in typical cases and in very many cases, in conjunction with bodily activities of the type which we designate as *reactions*: that, for example, the awareness of a colored object is strikingly bound up with such reactions as touching the object, moving the head, or with reactions of the visual organs. Second, that there are two distinguishable ways of being conscious of a thing, one of which we commonly designate as *perception*, and the other way as *imagining* or *thinking*.

Third, that perceiving and thinking alike have in very many cases a peculiarity which we roughly describe as personal; *i.e.*, that each occurrence of awareness tends to have something about it, a "reference," which we express by saying not merely "perceive," but "I perceive."

Now these are empirical facts. And by continued observation we make other and more detailed observations of fact. But it is important to note the limitations of that which we observe, and the ease with which we pass from these to assumptions which we subsequently confuse with observed facts. Descartes, for example, after pointing out the personal nature of consciousness, assumed it to be a universal peculiarity, and assumed that the "I" has certain characteristics such as *substantiality*, which are not empirically observable. Others have assumed that the *awareness* (which is the abstract term for the several acts, or facts of being aware) is itself a thing, and capable of being an object of awareness. Others have assumed that the *awareness* and the thing *of which* I am aware are identical. These assumptions, upon being made, lead to systems of psychology which might possibly be true, but which are not empirical. The assumption of the identity of the *awareness* and the *thing of which*, has had especially serious results, since it permits the substitution of the one for the other in reasoning, leading to complicated conclusions which have only conjectural validity, and hence make the whole system conjectural.

Scientific psychology, because of its compulsion to be scientific, must avoid these *assumptions* of fact. And yet it can not advance with the mere observation of detached and fragmentary details. It, therefore, having established itself on a scientific empirical *basis*, adopts the scientific *method* to which it is now entitled, namely, the framing of *working hypotheses:* and the progress of psychology, like that of all other sciences, is solely a matter of the framing, modifying, and rejecting of working hypotheses, on the basis of empirical observation and through close logical reasoning.

The primary working hypothesis of modern scientific psychology, arrived at by a long series of modifications, is the *reaction hypothesis*. This hypothesis is to the effect that awareness is always conjoined with reaction, or with vestigial reaction. This second clause is not to be neglected, as the hypothesis is not valid without it. There are various ways of expressing the hypothesis, which are practically equivalent, but which have differing appeal for different psychologists. Thus, I prefer to say that awareness is *caused* by reaction: but those

who have a definition of cause which differs from the one I would accept may object to that statement.

This principle, however we may formulate it, is a *working hypothesis*, and nothing more. But with prolonged observation failing to show positive exceptions, and with verification by the usual scientific procedure of *deduction and test*, it becomes a fixed principle, valid, like the law of gravity, so long as we acknowledge its nature; and it is subject, like that law, to modification when facts are found which indicate the necessity of modification.

The scientific results of this hypothesis are far-reaching; explaining phenomena already observed; initiating research along various lines in which the consequences of the principle can be deduced and put to test; and correcting inadequate conclusions of the past based on less adequate hypotheses. Into the details of these we can not here go, but I may point out as an extreme example, that "innate ideas" which were so impressively demolished by Locke, are now by no means to be ignored, but that on the contrary, their existence is quite probable, as biological analogies suggest they should be. Other important consequences I shall mention later.

The empirical basis of scientific psychology excludes the Malebranchian dualism of mental object and physical object, and with it the tedious sham battle between "interactionism" and "psychophysical parallelism." It also removes the bugbear of solipsism, which has darkened the lives of not a few psychologists of the past. These results are not due to the working hypothesis, although strongly consistent with it, but are the direct result of insistence on *empiricism*. In these respects a concrete progress is attained at once, and hence we may reasonably designate attempts to revive the psychology of "mental objects," or "observable mental states" as reactionism, and like all reactionism, a strong ally of radicalism, which in this case is represented by behaviorism.

Scientific psychology, as I have been describing it, is the legitimate and desirable outgrowth of the scientific progress of the past, and neither on the one hand a revolutionary program, nor on the other, an undue reliance on the accomplishments of our fathers, which no longer suffice in the changing order of social needs. And it is the social need of the fostering of this scientific psychology of which I wish especially to speak to-night.

One of the social problems now confronting us, and one with which scientific psychology is especially competent to deal, is the enormous spread and influence of mysticism and pseudo-

mysticism. Perhaps the pseudo-mysticism of spiritualism, telepathy, and all the train of table-tipping, ouija-boarding, levitation and dowsing which goes with these, furnish the most spectacular anti-scientific demonstration of our times. Yet the growth and power of philosophical mysticism, with its definite principles, and fully oriented antagonism to scientific method, is possibly a more serious danger. If one takes account of the remarkable increase in the sale of the books of Underhill, Inge, and others who represent to-day the ancient theories of Plotinus and of Dionysius the Areopagite; if one considers the increased number of such books, and the active propaganda of the mystics in the magazines of supposed culture; one will be impressed with the fact that this system is at least as fully alive and influential as it was in the days of Meister Eckhardt and Tauler.

Philosophical mysticism makes its attack upon science through the rejection of the psychological doctrine of knowledge; and if it can succeed in overturning this, its demolition of the fundamental methods of all science is well begun. Psychology is, in short, the keeper of the gate against this enemy, and is responsible to the whole of science for its defense.

The psychology of the past has fought a good fight against mysticism: it is demanded of us that we fight a better fight. This we can do only by consolidating our ground and improving on the weapons which have so far served; we can not do it by throwing away our arms and abandoning our advantage. Scientific psychology is a consolidation of our ground, because it makes the scientific doctrine of cognition, hitherto somewhat vague, precise and definite, and knits it up with our great fund of acquisitions in biology. No one can accept the fundamental hypotheses of scientific psychology and be in the least mystical. We have progressed beyond the loose conception of a consciousness which may have two, or three, or four, or more different varieties within it, and which is confused by the baffling questions of parallelism and interactionism. We have developed the conception of a single sort of consciousness whose conditions are definitely known, and this conception is so linked with empirical facts and the fundamental working hypothesis as to preclude the possibility of a "third kind of knowledge." Moreover, the very facts of the mystic experience itself are so completely explained on the reaction hypothesis, that the mystics are made to testify to the futility of their theory.

Against the pseudo-mysticism of spiritualism and its occult train, psychology is again the defender of the scientific faith. So well has psychology maintained its honor in this respect,

that it is obvious to him who runs, that the study of psychology is the best possible antidote against occultism. The very fact that the company of scientific men who have fallen into the snare of the "strange woman" of occultism is conspicuously devoid of psychologists is significant. And the scientific psychology based on the biological reaction is more solid, and better prepared against this enemy than was the older psychology. Since it bases all conscious processes on stimulation, and makes thought, as well as perception, depend on reaction, it can more easily explain the lack of evidence for telepathy on the one hand, and on the other hand it can explain how the sincere medium can be deceived by the occurrence of expressions of which she is not conscious until she speaks or writes them. Through its keener understanding of the relation of motor and conscious processes, moreover, it facilitates the work of detecting the mechanisms through which the apparently occult effects are produced. Its fighting superiority in these regards, over the psychology which preceded it, is evidenced by the fact that it is never driven to refuge in the depths of the subconscious or unconscious, as the older psychology sometimes was.

In the fight against mysticism and pseudo-mysticism, psychology has a duty to perform, both to science and to society, which it can not shirk. And it can not perform this duty through devotion to the psychological arts. The highest development of mental testing, for example, though immensely valuable in other ways, can have no function in this battle. Behaviorism, on the other hand, is an avowed pacifist doctrine which abandons the battle from the start, by disclaiming any responsibility for that over which the battle is waged. If our enthusiastic devotion to the psychological arts diverts so much attention, and so much energy, from the scientific work in our laboratories and the scientific instruction in our systematic courses that we go back upon the work of our fathers, and forsake the sacred task of defending our sister sciences and our culture: if we neglect the duty that they have entrusted to us: then our gains are to be counted losses, and our pride in our accomplishment is a disgrace.

The demand for scientific psychology to withstand mysticism and occultism is after all a demand laid upon it as the representative of the historic stem of psychology, and it might be a question whether, in spite of the claims I have here made, it can fight this fight better than its mother, or whether it fights with any other weapon than those she has used with honor. We may indeed pass this point over, pointing out only that the

fight is laid at least upon the lusty child as well as its mother, and that psychological radicalism yields the fight altogether.

There is, however, another force arrayed against biological science and against social progress which nothing but scientific psychology seems able to withstand. This force is the so-called "new psychology" or *psychoanalysis* of Freud and his followers, which lays claim not only to the field of psychology, but also to the fields of literature, art, religion, history, politics, and archeology.

Psychoanalysis threatens the older psychology not so much with demolition as with absorption. Developing its doctrines on the very conception of a world of psychic objects or ideas which the older psychology postulates, it merely extends this conception in certain directions, giving to the ideas a little more independence, a certain energy in their own right, and a power of existence as psychic objects when one is unconscious of them. Psychology may object to this extension, but the legitimacy thereof can not positively be disproved without rejecting the conception of psychic objects themselves. Hence, we are not surprised to find that many of those who held to the older psychology have given up to the Freudians and have put on their uniform, and that those who have not given up seem to offer a futile resistance. The Freudian propaganda proceeds by leaps and bounds, and Freudian books can not be produced in great enough quantity, nor their prices be put high enough, to fill the demand. The Behaviorists here also are of no help in the fight. There is for them theoretically nothing to fight about, and as a matter of fact the majority of them seem inclined to join the enemy.

The infiltration of psychology by Freudianism proceeds so fast that already much of the established data of psychology has been appropriated and woven into Freudianism in such a way that the rank and file believe Freud originated all psychology. One is informed that no one ever supposed that these were definite causes of mental phenomena before Freud discovered them: it is even implied that the association of ideas was a Freudian discovery: and one who admits that sex has some importance in individual and social thought life is classed by the rabble as a Freudian at once.

The danger from psychoanalysis comes much nearer home to psychology itself than does the danger from mysticism and occultism. Interest is diverted directly from psychological research wherever Freudianism thrives. Experimental work is not encouraged by the Freudians, because they have a schematic

explanation of everything on *a priori* principles. Thus the support and encouragement which the experimental psychologist needs for the promoting of his work tends to be withdrawn where psychoanalysis rears its head as a rival. That this discouragement of, and opposition to, psychology reacts on the psychological arts, many of you know from your own experience.

But even here, I do not wish to stress the interests of psychology itself, but rather to emphasize its duty to general science and to society at large. In the present emergency, men of other sciences look to psychology for light and information concerning the large claims of psychoanalysis. And the light of psychology seems too dim, and its voice is too much enfeebled to carry far into the crowd. Upon us also is the responsibility to protect society from charlatanism and superstition. And if we allow psychology to become merely a synonym for Freudianism, we put ourselves in the position of silly sheep whose clothing the wolves are wearing.

Scientific psychology is the sure antidote for Freudianism because of its three essential characteristics: its empirical basis, its logical method and its fundamental working hypothesis that the fact of consciousness is uniformly connected with reaction. Refusing the *a priori* concept of a world of mental objects, and adopting the empirically given fact of *being conscious*, scientific psychology removes the ground on which the doctrine of "unconscious consciousness" is based; and by emphasizing in its working hypothesis the continuity and common ground of habits of action and habits of thinking, and their essential interrelation, scientific psychology explains the facts of conscious life, whether of waking life or of dreams, without leaving any function for the mystic "unconsciousness," even if one should wish to posit it.

Furthermore, by its insistence on the fundamental logical precaution of using a term only in a definite and uniform sense, scientific psychology destroys the method of procedure which Freudians borrow from the mystics, which method consists in employing the well-known logical fallacy of the ambiguous middle term. Robbed of the easily shifting meanings of *libido, sexual, unconscious*, and the other stock terms, and with the light turned on its *a priori* basis, the Freudian system goes to pieces inevitably.

These needs for psychology which I have been presenting are of a definite, if negative, nature. Psychology is needed to ward off the assaults of definite forces directed against the in-

tegrity of science and the intellectual values in society. But there are other needs, of a positive sort, which unfortunately are less definite although no less important. These needs are more strictly social than the ones we have been considering.

Society is in a state of extreme restlessness. Social problems are perhaps no more pressing today than they have been in every period, but the recognition of these problems is certainly more clear. Our political system is being questioned and attacked. There are those who proclaim in essays, in books and in editorials in our most respectable journals, that democracy is a failure, that the declaration of independence is an absurdity, and that the rights of man are a nuisance. Compared with the feeble efforts of the Bolshevists, these attacks upon our governmental system are enormously more unsettling.

Aside from the shaking of the foundations of government, our whole attitude towards laws, and the making of laws, is in flux. And our conceptions of economic principles and economic ideals are not as firmly fixed as they were. People are seriously questioning the rights of property, so long held sacred, and asking whether it is not necessary for social welfare that these rights should be modified. The question of population is being given a new sort of consideration, and immigration and the birth rate are being made problems dependent upon principles of eugenics and the raising of standards of comfort and culture, rather than matters of religion and pride of magnitude. There is a growing tendency to treat all social problems, including problems of economics and political problems, not as mere matters of blind principle, but as matters for scientific and ethical determination. And men are seeking for light on these problems, and for relief from prejudice, as they have never sought before.

In this situation it is well that we should ask ourselves: What is psychology doing for the advancement of the solutions of our social problems? The answer is—nothing. Most of you perhaps think that psychology can do nothing, and that no responsibility rests upon it. You will undoubtedly agree with me that while clinical psychology and criminal psychology, and allied branches, have great value as sources of ameliatory measures in the conditions which confront us, they offer nothing towards the basic improvement of these conditions. The psychological arts contribute to the solution of our social problems no more than do the mechanical arts or the industrial arts, but rather contribute with these to make the problems more acute.

Where, we ask, is our social psychology? And the answer

is, that it is yet in the making, and that it is being made not by psychologists, but by politicians, and by independent thinkers like Bertrand Russell and Bernard Shaw. And if you ask my opinion, I will tell you that I think it is being very imperfectly made, as we should expect under the circumstances.

We should have a social psychology. Psychology should have something to offer to the world on these problems which are by admission of all so largely psychological. And there is no school of psychology to which we can turn hopefully except that which I have been describing as scientific. The older psychology has acknowledged its impotence. What it offers as social psychology is merely the pleasant diversion of understanding social events after they have happened. But this is not to be charged against that psychology as a crime. Rather, the traditional psychology is to be given the major credit of having led up to the point at which it seems possible that we may, if we carry out the trust it has confided to us, offer a real social psychology.

On this point, as on the others, the more radical psychology, which abandons the parent stem, is found wanting. That which is needed is a social psychology: not physiology. We need to deal with the desires, with the purposes, the emotions, and the conscious tendencies of men and women, and we need to deal with them as they are; not after abstracting from them that which is their important and essential characteristic.

The scientific psychology—empirical: omitting nothing from the facts: assuming nothing as empirical fact—logical: arriving at no conclusions by juggling with terms—proceeding experimentally with working hypotheses—alone offers any chance of finding the help which society needs. And in scientific psychology, treating men as they are, as active conscious beings, without building up around itself a wall of psychic objects, there is real hope. Of this I am firmly convinced, although I can not demonstrate the certainty.

There is, then, great need for scientific psychology, and unless the scientific study of psychology is promoted, in laboratories, and in the field: and unless the teaching of scentific psychology is fostered in the class room, and in print, culture will be impaired, and social progress impeded.

In emphasizing the need of scientific psychology, I repeat, I do not wish to be understood as ignoring the claims of the past to recognition and appreciation. On the contrary: the possibility of a scientific psychology to-day is founded on the work of Aristotle and his successors through the centuries. But I wish

to repeat, that we are false to our predecessors, if, after having received the brand from their hands, we stand still and do not go forward with swift feet.

Again I must insist, that I do not minimize the importance of progress in those psychological arts in which we are so much concerned. But I do insist that there are values for which mental measurements and the other arts do not assume the responsibility, and that the progress and the social worth of these psychological arts themselves are dependent on these other values.

Finally, I must explain, that, however narrowly I may have defined scientific psychology, I stand upon no narrow definition and no rigid test. I believe that in spite of differences of formulation, differences of minor principle, and even differences of purpose, the great body of American psychologists are in spirit united in what I have imperfectly outlined as scientific psychology, and that we shall overcome the dangers to which I have pointed, and will go forward not only to continued scientific achievement, but also to that essential social service which psychology alone can render.

JOSEPH LISTER, HIS LIFE AND WORK[1]

By Dr. PAUL F. CLARK
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JOSEPH LISTER was born on April 5, 1827, the son of Joseph Jackson Lister and Isabella Harris. His father was a London wine merchant of good old Quaker stock and a man of unusual ability. He devoted his leisure hours to the study of optics and was to a degree the founder of modern microscopy in that he was the discoverer of important equations which led to the production of the achromatic lens. Joseph Jackson Lister was largely self-taught and in the midst of an active business life he found opportunity to make his mathematical calculations, to grind lenses himself, and to earn a reputation which gained for him fellowship in the Royal Society in 1832. He was also a good Latin scholar, a skilful artist, and well versed in French and German.

Joseph's mother was a cultivated and beautiful woman of strong character who apparently, however, had less influence on the development of the future surgeon than had his father.

Shortly after his marriage, the elder Lister bought a commodious attractive house with fields and gardens at Upton in Essex. At that time, Upton was only a winding lane with a scattering of comfortable houses instead of a sordid part of busy London as it is now. Forest, marsh and the delightful banks of the Thames made a peaceful rural setting for the boyhood of Lord Lister. Upton was a community of the Friends and there young Lister grew up with his three brothers and two sisters among the rigorous restrictions and in the peaceful isolation of such a group.

BOYHOOD AND EDUCATION UNTIL 1852

Joseph Lister went to two private schools; the first at Hitchen and the second at Grove House, Tottenham. He was rather a precocious, serious lad, especially well versed in the classics because of his training at home with his father. The children used to read Latin classics to the elder Lister while he was dressing. On the whole, however, his amusements were those of an ordinary schoolboy. More than thirty of his early

[1] Paper read before Wisconsin Historical Seminary May 26, 1920.

school papers are still preserved. It is interesting to note that four of these dealt with the human skeleton and one on the similarity of structure between a monkey and a man. At an early age he became interested in natural history, dissecting many small animals and articulating skeletons. While still young he decided to be a surgeon and never deviated from his chosen path.

Lister's religious affiliations debarred him from entrance into the older universities, so after completing his preparatory course at the age of seventeen, he was sent to University College, London, which was distinctly non-sectarian. He spent three years there working for his A.B. in rather gloomy surroundings.

He began his early medical studies in earnest in the winter of 1848, devoting more of his time than does the average medical student to the fundamental sciences, including physics. Lister's introduction to surgery came shortly after the discovery of anesthetics, and although this discovery represents a striking landmark in the history of surgery, the change produced came only gradually. Operations were still performed at high speed and the technique used was the same as found in the pre-anesthetic days.

> At University College Hospital one theatre of modest dimensions containing one small instrument cupboard, a sturdy wooden table, a single gas jet and a solitary washing basin served for every purpose including the novel one of administering the anesthetic.

In following the cases Lister observed that it seemed to be a lottery whether patients recovered or died. Inflammations, suppurations, erysipelas and hospital gangrene were the too common sequellæ of operations. In such surroundings, then, Lister obtained his first impressions of the profession which he had chosen for his life work. His cousin, Thomas Hodgkin, described him as "kind and considerate but rather dwelling apart and not making any strong friendships with his fellow students." But an older member of the group of Friends with whom they both lived spoke of him as one "who in the total excels any one I know, or have known, in bright promise for the future."

When he became a resident in the hospital, he was associated with men of other religious denominations and from other schools and thus began to have a somewhat broader outlook on life and to enjoy the pleasant club life of the hospital interne. He entered with zeal into student politics and the debating society, heading a sharp attack on the homeopaths.

He was granted the degree of M.B. of the University of London and a Fellowship of the Royal Society of Surgeons in 1852 at the completion of nine years at the University College.

During the latter part of his course, he did some original work stimulated by Wharton Jones and William Sharpey, professor of ophthalmic medicine and surgery and professor of physiology, respectively. His observations were published the next year, 1853, in the *Quarterly Journal of Microscopical Science*. The first of these papers confirmed and extended Kölliker's observations demonstrating the existence of two distinct muscles, the dilator and sphincter in the iris. The second paper also confirmed the work of Kölliker on the structure of the involuntary muscle fibers of the skin. These papers are beautifully illustrated with his own camera lucida drawings. These investigations were carried out before any of the modern methods of sectioning and staining were known, but in spite of this, the work is surprisingly well done.

Holidays were not neglected during his student days and these he spent usually with members of his family in different parts of England, Ireland and the Continent. He kept elaborate leather-bound diaries, most of which have been preserved. The style in his diaries is rather stilted and the subject matter not especially interesting. He shows, however, that he was a keen observer of geology and architecture, manners and customs, and that he had acquired considerable facility with French and German. Two sports that attracted him were swimming in the summer time and skating in the winter, although as to the latter he states that he never got beyond the making of figure eights and threes of rather small dimensions.

EDINBURGH, 1853–1860

At the end of his medical course, Lister, having no definite plan in mind, was advised by Sharpey, who had influenced him markedly during his medical course, to visit Syme in Edinburgh for a month and then to make a more prolonged trip to the various continental schools. So in September, 1853, Lister went to Edinburgh, presented his introduction to Mr. Syme, and was received most cordially. Syme invited him to his home, gave him the opportunity of assisting with private operations, and set him to work in the hospital. Lister was surprised to find that Edinburgh was ahead of London in many ways. Syme was at this period fifty-four years of age, in the prime of his work and was, perhaps, the first of British surgeons. He was a pioneer not only in the manner of teaching

clinical surgery but in the wider field of general medical education. There seems to have been a mutual attraction between Syme and Lister and, persuaded by his admiration for Syme and the unusual opportunities offered him, Lister determined to remain in Edinburgh. A month later he was made supernumerary house surgeon and soon afterwards resident house surgeon. This gave him ample opportunity, as Syme seldom interfered with the treatment of ordinary cases and accorded Lister the exceptional privilege of using his own discretion as to which of the cases admitted at night he should himself operate upon. Lister became a frequent visitor at Milbank, where Syme devoted his leisure to his garden and his orchids. There he met famous men from all parts of the world, as Syme was one of the most prominent in this important medical center. In the autumn of 1854, Lister applied for the vacant assistant "surgency" at the Infirmary. Syme promised him his support and also free access to his wards. Accordingly he hired the lecture room and began preparing for the lectures in earnest. Only four months remained before the opening of the winter session and there were many duties and distractions. One of the greatest distractions was Syme's eldest daughter, Agnes, and by the end of July he found he could do nothing more in the preparation of his lectures until the matter was settled. Fortunately it was settled favorably. One difficulty arose, however, in that Miss Syme was not a member of the Society of Friends and Quakers were not allowed at that time to marry "Out of the Society." Accordingly Lister resigned and became a member of the Church of England. This change never seems to have caused him any mental unrest.

The marriage took place at Milbank on April 23, 1856. The young couple spent a month in the region of the English lakes and at Upton and then started on a three months' tour on the Continent. In the course of this trip Lister made the acquaintance of many surgeons, visited most of the leading medical centers of Europe, and greatly improved his facility in the use of the Italian and German languages. In Vienna he met Rokitansky and saw his remarkable pathological museum.

Lister and his wife returned to Edinburgh in October, 1856, and settled in their new home, 11 Rutledge Street. This was not very attractive as to its surroundings, but was near the business houses and well adapted for consultations. He was elected to the assistant "surgency" in October, 1856. The number of students attending his lectures this year was only eight, but he was not at all discouraged. At this time he was

hard at work on his investigations on the early stages of inflammation and some experiments upon the coagulation of the blood. He read a paper before the Royal Society of Edinburgh on December 1, 1856, "On the Minute Structure of Involuntary Muscular Fiber." This again confirmed and extended some observations of Kölliker. During the early part of 1857 his letters refer to the excitement of his first public operation at the Infirmary and to the beginning of his private practice. He was delighted with his work and in a letter to his mother his attitude is shown by the statement that "fearing as I am sometimes apt to do that a mode of life so much in accordance with my tastes as mine now is must be too pleasant to be proper for me." In a letter to his father, February 26, 1857, he describes his feelings at his first operation:

> Yesterday I made my debut at the hospital in operating before the students. I did two operations in the presence of a very full theatre and several surgeons and old practitioners. I felt very nervous before beginning, but when I got fairly to work this feeling went off entirely, and I performed both operations with entire comfort. And I also explained the operations and cases to the students without embarrassment. Altogether I felt very thankful at the way I was able to acquit myself. Everybody congratulated me afterwards and the students cheered me very warmly.

In June, 1857, he read an important paper on the early stages of inflammation. He made the observations for the most part on the frog's web, although he also used the bat's wing in order to utilize a warm-blooded animal. He describes very clearly the vascular changes in early inflammation, using a great variety of irritants such as hot water, mustard, etc. He apparently never detected the migration of white blood corpuscles, which was due probably to the fact that each experiment extended over a very short period of time. Cohnheim, to whom the credit is usually given for the earliest observations on the microscopic changes in inflammation, did not publish until ten years later, 1867. As a matter of fact, credit really belongs to neither of these men, but to William Addison (1843) and Augustus Waller, who made careful observations in 1839, although he did not publish until 1846.

In the summer of 1858 he started another course of lectures dealing with surgical pathology and operative surgery. His desire to give his students the fundamental facts of physiology led him to work still further on the coagulation of the blood. In his collected papers are five dealing with this subject.

During all this period Lister worked most industriously, frequently far into the night, on his experiments. Mrs. Lister

assisted him greatly, taking his dictation and aiding him with the details of his work.

Lister's private practise was growing, largely through the influence of Syme, and he was enjoying the increased prestige of his position. He was looked upon as a young surgeon of great promise and a first rate investigator. In August, 1858, the professorship of surgery in the University of Glasgow became vacant and Lister was urged to apply for the position. It was a regius professorship, and although there was considerable difficulty and vexation due to politics, Lister's appointment was finally made on January 28, 1860. The seven years in Edinburgh were accordingly brought to an end with much regret on the part of Lister, his students, and colleagues.

GLASGOW, 1860–1869

The appointment at Glasgow offered a wider opportunity for his powers than could possibly have been the case at Edinburgh at that time. In Edinburgh he was somewhat overshadowed by the strong personality of Syme, whereas in Glasgow he was now thrown upon his own resources. For the first year he was without any hospital appointment, but had plenty of private practise and came into close association with many congenial colleagues. His induction into office involved the presentation of a Latin thesis which he sat up practically all night to write and finished on the train from Edinburgh to Glasgow. This was a common habit of his. There were always many demands upon his time, and he never allowed himself time enough to write his lectures or prepare for the extramural functions that were a part of his duties. He was not given to punctuality. The matter at hand was always of greater importance than the duty of the future.

The first lecture of the winter session at Glasgow was on the importance of surgery.

> He dwelt on the value of anatomy and physiology to the surgeon and amongst other surgical matters, on the importance of making serviceable stumps which he illustrated by a case of amputation of both legs in which his patient was still able to dance the Highland Fling.

The number of students in his class finally reached 182, probably at that time the largest class in systematic surgery in Great Britain. The students became very enthusiastic, made him honorary president of their medical society, and backed him in his claim to the "surgeoncy" of the Infirmary with 161 signatures. He was finally appointed surgeon to the Royal In-

firmary on August 5, 1861. In all his teaching, whether in the form of lectures, demonstrations, or the correction of student papers, he was exceedingly conscientious. In correcting examination papers, every point in an answer to a question was given its percentage value and the papers were graded with painful precision.

In spite of daily lectures and the pressure of private practise, he published at this time an important paper on the excision of the wrist for tuberculosis, which extended his reputation very considerably.

In September, 1864, Lister's mother died. The parents had been living alone at Upton for the previous six years and after the death of his wife Joseph Jackson Lister passed the remaining five years of his life in lonely isolation. His son, Joseph, from this time on, wrote a weekly letter to his father, and it is from these letters that we gain much information in regard to the daily life, ideals, work and ambitions of Lister.

In order the better to understand his most important work, namely, Lister's discovery of the principles of antiseptic surgery, it will be necessary to suggest briefly the conditions that prevailed in hospitals. At that time there was no security that the simplest operation would not end in a fatal septicemia. In Glasgow and in fact in all the cities, wards were not infrequently closed for a time on account of the frightful mortality. In Nuremberg the hospital authorities seriously considered the pulling down of the Allgemeines Krankenhaus. Microbes were looked upon as scientific curiosities, the hospital diseases, erysipelas, pyemia, septicemia, hospital gangrene, and tetanus were often called septic, but no one realized what that meant. "Surgeons were fighting all of these diseases in the dark or submitting to them as inevitable." These were the days of "laudable pus" and the common assumption was that putrefaction and purulent infections were due to the oxygen of the air.

Surgical cleanliness as understood at that time would astonish a modern observer. In the institutions that had felt the influence of Florence Nightingale, the nurses indeed were fairly neat and esthetically clean.

But no such attempt to satisfy the proprieties was made by the surgeon and his assistants. When a dresser or a house surgeon entered upon his term of office, he hunted up an old coat in the lapel of which he probably carried a wisp of ordinary whipcord for tying arteries. This garment did duty for six months or a year and was then very properly discarded. There were no such time limits, however, for the surgeons themselves. Their operating coats lasted from year to year and event-

ually acquired an incrustation of filth of which the owners appeared unconscious or even proud. This set the tone and some who were then young can remember the scorn with which they were greeted when in their reforming zeal they broke away from ancient custom, boldly took off their coats and operated with upturned shirt sleeves.

No attempt was made to isolate septic cases; nurses and dressers passed directly from erysipelas wards to healthy patients. Lister complained that in Glasgow the closets communicated directly with the wards. The supply of water even in the operating theaters was inadequate and one of the frequent duties of the operating surgeon was the performing of postmortem examinations. As in Vienna in the time of Semmelweis, the surgeon used frequently to come from the postmortem chamber directly to the operating room, with only such cleanliness as common decency demanded. Surgeons were not ashamed to speak of a "good old surgical stink."

The mortality statistics after amputation gives some indication as to the prevailing conditions. At the Edinburgh Infirmary the death rate was 43 per cent.; at Glasgow Infirmary 39.1 per cent. At the Pennsylvania Hospital the record was unusually good, the average mortality for a series of years being 24.3, while at the Massachusetts General Hospital, the average was 26 per cent. The mortality in the French Hospitals was even greater than that in London, amounting to about 60 per cent. Billroth at Zurich reported a mortality of 46 per cent. Sir James Simpson, the discoverer of chloroform anesthesia, stated "that the man laid on the operating table in one of our surgical hospitals is exposed to more chances of death than the English soldier on the field of Waterloo."

The cause of these conditions was unknown; doubt and uncertainty existed in the minds of all. A polluted atmosphere was the generally accepted cause, although opinions differed as to how the air became tainted. The work of Semmelweis (1861) had made no impression and it remained for Lister to rediscover and extend the observations of this martyr to science.

In handling open wounds at this time, two general lines of treatment were followed. One was to encourage the formation of a scab. After the edges of the wound had been approximated, no dressing was applied, but sometimes certain powders or caustics or a piece of lint soaked in compound tincture of benzoin. The other method was known as the open treatment. The edges of the wound were allowed to remain quite open and a piece of linen protected it somewhat from flies and dust.

Some carried the first method to the point of what is called the "occlusion method." Collodion was used, completely excluding the polluting air. The occlusion method obviously did not allow the drainage of pus, so Syme adopted what he called a dry dressing. Lister pointed out, however, that this very soon became a moist dressing due to the discharge from the wound. Water dressings and bread and linseed meal poultices were in common usage in some of the hospitals. Ligatures were of silk or whipcord drawn over beeswax. Usually one end was cut off short and the long ends of several ligatures gathered together for drainage. With the progress of the purulent inflammation which so commonly occurred, the ligatures were sloughed off and secondary hemorrhage was common.

One more point deserves attention, namely, "subcutaneous" surgery, by which was meant the performance of a minor surgical operation through a puncture so small that little air was admitted. The fact that these operations were so commonly free from putrefaction was held to be due to the fact that air was not admitted to the wound.

It was under such conditions as these that Lister taught and worked, his technique being no better than that of many others. There was this difference, however: he did not believe these conditions inevitable, and he was constantly searching by observation and experiment to find out the cause of the high mortality in all hospitals.

Sepsis was known at the time, though the cause was unknown and the word "antiseptic" occurs in medical literature as early as the middle of the eighteenth century. Many antiseptic substances had been used from the time of the early Egyptians and doubtless before that. At the time of which we are writing, alcohol was freely used on the continent and it was of course the basis of all tinctures. The good Samaritan poured in "oil and wine." Glycerine was used in England, especially in cases of hospital gangrene. Chlorine and its compound, chlorinated soda, and chlorinated lime were early recognized as powerful antiseptics. Iodine was discovered in 1811, was used for a while, and then went out of fashion. In 1815 the antiseptic properties of coal-tar products were known in France and carbolic acid was discovered by Runge in 1834. As early as 1851, Calvert, an English chemist, used carbolic acid for the preservation of cadavers. Jules Lemaire worked extensively with carbolic acid and published in 1860 and again in 1863. Carbolic acid in the treatment of wounds and sores became the fashion in France for a while. All of these anti-

septics were used, however, not with the hope of preventing the occurrence of putrefaction, but with the aim of neutralizing its effects after it had developed. The two exceptions to this rule were Semmelweis in Vienna and possibly Lemaire in Paris.

In 1865, while intensely occupied with the study of suppuration, Lister learned from the work of Louis Pasteur that putrefaction was a kind of fermentation caused by the growth of microorganisms and that these microorganisms were present in the dust of the air and responsible for wound infection.

The enormous significance of the work of Pasteur was immediately apparent to Lister, who had for so many years been struggling with this problem. Since the cause of suppuration and septicemia was now known, it was obvious that the prevention of such infections depended upon keeping the causative organisms away from the wounds. His attitude can best be understood by quoting from one of his papers published in 1867.

> In the course of an extended investigation into the nature of inflammation and the healthy morbid conditions of the blood in relation to it, I arrived several years ago at the conclusion that the essential cause of suppuration in wounds is decomposition, brought about by the influence of the atmosphere upon blood or serum retained within them and in the case of contused wounds upon portions of tissue destroyed by the violence of the injury.
>
> To prevent the occurrence of suppuration with all its attendant risks was an object manifestly desirable; but till lately apparently unattainable, since it seemed hopeless to attempt to exclude the oxygen, which was universally regarded as the agent by which putrefaction was effected. But when it had been shown by the researches of Pasteur that the septic property of the atmosphere depended not on the oxygen or any gaseous constituent, but on minute organisms suspended in it, which owed their energy to their vitality, it occurred to me that decomposition in the injured part might be avoided without excluding the air by applying as a dressing some material capable of destroying the life of the floating particles.
>
> Upon this principle I have based a practise of which I will now attempt to give a short account.

His notion then was much more like that of aseptic surgery than is generally conceded. He likened our skin to the glass of Pasteur's flasks. In a compound fracture the bottle is broken and the air with the attendant bacteria gain access to the tissues below. To be sure he paid more attention to air as a source of infection, although he recognized that his hands and open wounds were also infected.

In considering the possible methods of eliminating the airborne infections, he chose chemical antiseptics as his means and happened to hit upon carbolic acid first. The first product

used was a crude acid known as German creosote, which he tried on a compound fracture.

Quoting from one of his papers,

> After cleansing the broken limb and squeezing out as far as possible all parts of blood, a piece of calico or lint soaked in undiluted carbolic acid and held by a pair of forceps, was introduced into the wound and passed freely in all directions in order to destroy the germs that had entered either at the time of the accident or afterwards and might be lurking in deeper parts. A piece of lint similarly soaked in the acid was then applied to the wound just large enough to overlap it in every direction by half an inch. This was covered by a slightly larger piece of thin block tin or sheet lead, the object of which was to prevent the evaporation of the antiseptic. The dressing was then fixed in position by strips of adhesive plaster and some suitable absorbent material was packed around between the limb and the splints for the purpose of soaking up any blood or discharges that might escape. The blood and carbolic acid soon formed a tenacious crust or thick paste which was not removed for several days but its antiseptic properties were renewed from time to time by painting some more of the undiluted carbolic acid on its outer surface after removing the metallic plate for the purpose.

He found that the crude acid caused necrosis and sloughing, so he next used a 5 per cent. watery solution of a purer product which he succeeded in getting and a higher concentration in linseed oil. This last was too irritating, so after repeated attempts he used a putty made with calcium carbonate and a solution of phenol in linseed oil, one part in four or one part in six. This did not irritate the skin or tissues greatly. He constantly reiterated that the ideal dressing should be mild and non-irritating and he continually advanced as his ideal the establishment of conditions in compound fractures and open wounds similar to those that obtained where the skin is unbroken. He did not obtain another suitable case for the application of his principle until the spring of 1866. This also was a compound fracture and he wrote his father on May 27:

> It is now eight days since the accident and the patient has been going on exactly as if there were no external wound, that is as if the fracture were a simple one. His appetite, sleep, etc., good, and the limb daily diminishing in size while there is no appearance whatever of any matter forming. Thus a most dangerous accident seems to have been entirely deprived of its dangerous element.

He published his epoch-making observations in several sections in the *Lancet* between March and July, 1867. Only one out of eleven cases died (although one had lost his leg), an almost unheard of success, especially under the abominable conditions that prevailed in the wards of the Glasgow Royal Infirmary.

From this time he continually strove to improve his tech-

nique, trying many different antiseptics and working with intense zeal on each case that came to his wards.

Lister claimed to have discovered a new principle necessitating fundamental changes in the conception of the causation of infected wounds and therefore in the treatment of these wounds. This claim was received with incredulity on the part of some, open resistance on the part of others, and apathy on the part of most persons. Unfortunately the nature of his discovery was from the first confused with the discovery of carbolic acid, and when it was made clear that carbolic acid had been used for years many considered Lister's claims palpably false. Sir James Y. Simpson led the attack on Lister in Scotland, and an anonymous letter in the *Edinburgh Daily Review* made it appear that all Lister had done was to reproduce in Great Britain what had long been the continental practise. The controversy waged rather bitterly at times and Lister suffered keenly from mortification. Attention had been directed to the use of carbolic acid and not the fundamental underlying principle, and in spite of all Lister could do, the phrases "carbolic treatment" and the "putty method" were on every one's lips. Simpson gave all credit to Lemaire and because of his extended reputation, this point of view became widely disseminated. Lister's chagrin can well be imagined, but in this short paper we can hardly afford the time to quote from his published letters on this subject. Very wisely he did not allow himself to be drawn deeply into the controversy, but continued his efforts toward the improvement of his technique, and in open lecture and published paper continually reiterated that his discovery was a fundamental principle and not a new method of dressing.

His successful treatment of cases continued. Visitors from other countries as well as from near by came to his wards for purposes of observation. Those who came and actually saw seem to have been well satisfied, and year by year his teaching was spread by his own students as they passed out to new fields of endeavor. He no longer used strong carbolic acid, but found a 5 per cent. solution adequate. Even at this time he was trying to dispense with antiseptics. He states:

> Of all those who use antiseptics in surgery, I expect that I apply them least to the surface of the wound.

At this time also he was attempting to find a suitable "protective," a non-stimulating substance impervious to carbolic acid; varnished oiled silk was his final word in this direction.

Secondary hemorrhage was a continual source of danger in

the septic wounds of that day and Lister was the first to recognize why the ligatures sloughed away from such a wound. He did much experimental work in attempting to find a ligature which should be strong enough and yet capable of being digested finally by the fluids of the body. Silk ligatures soaked in strong carbolic for two hours, catgut ligatures soaked in a great variety of disinfecting solutions, were used one after another. His final process in 1908 was the use of chromium sulphate and corrosive sublimate as a means of disinfecting well-seasoned catgut. This and several other methods of treating catgut devised by Lister are still in use to-day.

In the spring of 1869 Syme had a paralytic stroke and in the summer he resigned his chair, urging Lister to become a candidate for it. A large number of the Edinburgh students sent a delightful letter to Lister urging his candidacy and on August 18 he received news of his election to the Edinburgh professorship. A month after this appointment Lister's father, almost eighty-four years old, was taken ill and shortly afterwards died. For Lister, this meant a deep personal loss and led to the breaking up of the old home at Upton.

EDINBURGH, 1869–1877

The Listers moved to Edinburgh in October, 1869. He settled at No. 9 Charlotte Square in one of the most fashionable parts of the city and had to pay what he called "a most enormous sum" for it. His private practise developed very rapidly, his lectures were largely attended by an enthusiastic group of students and he was delighted to find his expectations realized in that his life was not as tied up with routine as at Glasgow. His university duties were considerably less, inasmuch as he gave only two clinical lectures a week, instead of the daily lecture in systematic surgery. He also enjoyed the fact that his hospital visit occurred at noon in Edinburgh instead of the early morning as at Glasgow.

He found many changes in Edinburgh from the conditions that had prevailed ten years before. Syme and Simpson, the two strongest personalities, were both nearing the close of their active lives. Syme indeed died in June after Lister's return to Edinburgh. He mourned the loss of Syme very deeply. The relation between the two had been almost that of father and son. Lister had always trusted much in Syme's advice and in his judgment, but with the death of Syme, Lister stepped into the first place among the surgeons of Scotland.

Lister began again to devote more of his time to investigations and bedtime was frequently postponed until four or five o'clock in the morning, while he made careful notes or delicate camera lucida drawings. His hospital visits occurring at noon caused the noon-day meal to be decidedly a movable feast. Each antiseptic case received his personal attention, and the assistants frequently became very hungry while their "chief" attended to details which seemed to them very minute. The classes were large, sometimes numbering 170 to 180, and the lectures, judging from the reports of his students, were intensely interesting. He no longer spent hours in preparation for his lectures, but perhaps half an hour sitting quietly in his armchair, thinking.

His laboratory work for the next few years was largely bacteriological in character. Four or five hundred closely written foolscap pages with many careful drawings in what he called his "common-place" book give evidence as to the large amount of time spent in bacteriological work. Altogether he published six papers dealing with bacteriological subjects, no one of which gives evidence of observations of lasting importance. They did, however, bring him into touch with Louis Pasteur and the correspondence between the two is delightfully interesting.

Two modifications of his surgical technique were introduced during the early years of his second Edinburgh period. The first was the carbolic acid spray, about which so much has been said in open ridicule and the second the use of absorbent gauze dressings. Pasteur's teachings, it must be admitted, strongly favored the air-borne theory, and there was at that time no clear-cut distinction between pathogenic and saprophytic organisms. In the use of the carbolic spray, Lister allowed his theorizing to lead him away from his experimental basis. In this period of his work it seems to me that he rather neglected his earlier fundamental conceptions, becoming infatuated with carbolic acid as a panacea. So in the minds of many the term "carbolic acid treatment" was superseded by the "spray and gauze treatment." Lister's notion was that the spray would mechanically cleanse the air and destroy the bacteria present. The machine for the production of the spray passed through a regular evolution from a small hand atomizer, to a foot spray worked with a bellows, to a cumbersome tripod machine worked with a long handle which bore the name of "the donkey engine" among the scoffers and finally to a beautifully designed machine producing a steam spray. The spray

was used in many parts of the world for years, although doubts arose in the minds of many and finally Bruns of Tübingen (1880) condemned it in a forceful paper entitled "Fort mit dem Spray." Lister finally abandoned the spray in 1887, acknowledged that its use had been based on a misconception of the facts, that pathogenic organisms were not very plentiful in the air, that it was extremely irritating to the hands of the operators, and that the air was not, after all, the principal source of wound infection. At the International Medical Congress at Berlin in 1890, he states:

> As regards the spray, I feel ashamed that I should ever have recommended it for the purpose of destroying microbes in the air.

The substitution of the gauze dressing for the plaster was a distinct improvement in Lister's mind, because it made the antiseptic treatment easier to carry out. Many different antiseptics were used to saturate the gauze and a great deal of experimentation was carried out to test each one. Oakum dressings, carbolic gauze, gauze soaked in corrosive sublimate, the double cyanide of zinc and mercury, all the different common salts of mercury, many volatile disinfectants, sero-sublimate gauze, and boric acid all passed through various stages of trial in his laboratory and wards. Several papers in this field were published in the *British Medical Journal* and the *Lancet* in the years from 1884 to the end of his active life as a surgeon.

The story of the gradual spread of Lister's teachings need hardly be recorded save in the briefest form. The controversy was waged and there were those who scoffed, but for the most part the junior physicians and surgeons everywhere were beginning to swing towards Lister's point of view. Among those on the continent who became his adherents must be mentioned Ernst von Bergmann, who became convinced that the antiseptic method was only a stage in the development of surgical technique and that the aseptic method must be the treatment of the future. He became the apostle of aseptic surgery and was instrumental in the general hospital improvement that dated from this period. Lister's teachings were accepted more rapidly in Germany, Switzerland and Scandinavia, than in England. Of the foreign countries France was especially slow to take up the modern methods.

During these years at Edinburgh his published papers were numerous. No less than ten connected with antiseptics and two on fermentation appeared between 1870 and 1876. During the Franco-Prussian War his hope that antiseptic methods

would lead to more adequate control of infected wounds was only partly realized.

In 1875 he made an extended continental tour with his wife and other members of his family. The first part of the trip, spent for the most part in Italy, was largely for pleasure, whereas the latter part, through the university towns of Germany, was a triumphal procession. He was dined and wined by physicians and students who welcomed him as the forerunner of modern surgery.

They returned to Edinburgh toward the end of June and immediately afterwards he took the first of many trips in opposition to the work of the anti-vivisectionists. This group of misguided sentimentalists became extremely active in England and, in spite of the opposition of Lister and other leaders in medicine and the biological sciences, put through a "cruelty to animals act" in Parliament which has hampered the efforts of experimenters in that country even to this day. Lister's letter to the Queen opposing the "cruelty to animals act" is a model of courteous but strong opposition to the restriction of well-handled animal experimentation. The law as passed forbids any experimentation save in a registered house by licensed investigators.

> It forbids the granting of a special license to a distinguished doctor to experiment on a chloroformed frog in his own study in pursuit of science, while it allows any one who can afford it to hunt a stag to death or set two greyhounds to chase a hare and wager money on the result.

In September, 1876, Lister, together with Mrs. Lister and his brother, attended the International Medical Congress at Philadelphia. Lister was made president of the Surgical Section, taking a prominent part in many of the debates. He traveled extensively both before and after the congress, visiting Montreal, Toronto, Niagara, going as far west as San Francisco.

In 1877 a chair of clinical surgery was created for Lister at King's College, London, and in spite of the vehement opposition of his students, Lister accepted it.

It is perhaps difficult to understand why Lister should have wanted to leave his preeminent position in Edinburgh, with his well-trained assistants in a hospital dominated by him, where he had large classes of enthusiastic and affectionate students and friends of long standing. But he left these desirable conditions to accept the position at King's College with a comparatively small London hospital, where the average class was less than 25 and where apathy or distinct opposition must be met daily. His primary reason for making the change was that

London was the only city of any prominence that had not adopted his procedures. He felt that he must carry the gospel to the Romans. He was a man with a mission.

LONDON, 1877–1893

In London the Listers lived at No. 12 Park Crescent, well away from the fashionable quarter and also remote from the recognized consultant's quarter, but near the Park Square Gardens and also the Botanical Gardens, and that was what Lister desired. He did not build up a large private practise in London. This was partly due to his lack of punctuality, and partly to his preference learned from Syme of leaving the amount of his fee to the patient's decision. This always placed the consulting physician in rather an embarrassing situation. He also did not wish to leave the after care of his patients to the physician consulting him, who usually had no notion as to Lister's methods. But he had just about the amount of practise he desired; patients were sent to him from all parts of the British Isles. He enjoyed immensely the personal relation established in private practise, taking the keenest interest in the affairs of his patients, and they in turn regarded him with veneration. He would have vigorously opposed our full-time clinical professorships just as at that day he opposed the notion that the professor of pathology should not have beds in the hospital.

He brought with him from Edinburgh four men to form the basis of his staff in King's Hospital. Without the assistance of these well-trained men he would have found it extremely difficult to do anything in the uncongenial atmosphere of King's College Hospital. With the nursing department, there was continued friction. The Sisterhoods were bound by their own notions and inflexible rules. His assistants, Watson Cheyne and John Stewart, tell of several amusing and almost tragic instances of the absurd obstinancy of the Sisters in charge.

One feature of Lister's methods of treatment is worthy of note in that it brings out Lister's great personal interest in his cases as well as the difficulties with the nursing organization. Chronic cases of tuberculosis of the bone, psoas abscesses, etc., which formerly were almost always fatal, became curable, but only with prolonged and careful treatment, so that some cases remained in the hospital for a long period of time. In October, 1877, the managers of the Edinburgh Royal Infirmary decided to discharge some of these chronic cases which Lister had left behind. One woman, Lizzie Thomas, came to London, as Lister

wished her admitted to King's College Hospital. She was brought by the Scotch head nurse in a basket such as were used to move patients in the Edinburgh hospitals. In spite of the fact that it was a cold bleak October morning, the basket and its contents were placed on the floor, while the Sisters insisted that the patient could not be admitted till the secretary arrived two hours later to draw up the regular papers for admittance. Not till Lister's assistant, John Stewart, threatened to put Lizzie to bed himself, with the assistance of the Crimean veteran porter, did the shocked Sisters reluctantly consent to take charge of the patient before all the usual formalities had been observed. In spite of her ungracious reception the patient recovered, as did the other six chronic cases whom Lister sent, at his own expense, to a nursing home, after they had been summarily discharged by the Edinburgh Infirmary.

He attempted to introduce the ward methods of teaching surgery that he had used in Edinburgh, but there were very few cases and the students were apathetic. Lister's classes remained small. Coaching classes were the order of the day and the London examination system which Lister vehemently opposed gave no power to the professor in charge of the course. Students found that Lister's views were not acceptable to the examiners, so they avoided his clinics, and instead of the 400 eager students as at Edinburgh, his lectures were attended by a bare dozen.

Gradually, however, the successful cases made an impression even in London and his lectures began to attract a few general practitioners and a goodly attendance of foreigners.

During his London period, Lister gave many addresses and attended numerous medical congresses. He was a juror at the University Exhibition at Paris in 1878 and seized the opportunity to become more familiar with Pasteur and his work. At the sixth International Medical Congress in Amsterdam, a great reception was tendered him, the ovation at his entrance lasting for some minutes. The seventh International Medical Congress met in London in 1881 and was a memorable occasion. Virchow, Volkmann, Pasteur, Billings and Huxley gave general addresses. The most remarkable paper at the meeting was given by Pasteur on "Vaccination in Relation to Chicken Cholera and Splenic Fever."

For fifteen years Lister occupied the chair of clinical surgery at King's College. His private duties were numerous and his public life became more and more complex. He received honorary degrees and various orders of merit from different

ruling monarchs. In 1883 he was made Baronet. His life in London, however, was less strenuous than that in Edinburgh. Experimentation went on as before and the "Commonplace book" is full of experiments on catgut, gauze, and various germicides. London was finally won over by the pertinacity of Lister and the gradual piling up of facts. Even the *Lancet* published laudatory remarks. He played more and more a leading part in public affairs and in medical education. He always opposed the admission of women to medical schools.

All this time, Pasteur had continued his remarkable studies on the production of active immunity by the introduction of attenuated organisms until one after another the diseases of chicken cholera, anthrax and finally rabies were rendered, if not harmless, at least less dangerous to man and other animals. In order to aid the work in the preventive treatment of rabies the Institut Pasteur was founded and for a number of years it was necessary to send all persons in Great Britain who needed treatment for this disease to Paris. Feeling that this condition should be rectified, Lister and others of the medical profession attempted to establish a similar institute in Great Britain. In spite of much opposition on the part of the anti-vivisectionists the British Institute for Preventive Medicine was established on June 5, 1891. This Institute, finally renamed after Lister, has gone through many vicissitudes but is still fulfilling the purpose of its founders.

As Lister's duties became less burdensome owing to advancing years, his vacations became more numerous and longer, although he never neglected this part of a normal life. He traveled widely, frequently making some medical congress the excuse for the trip. He visited Norway after the Amsterdam congress in 1880, visited Spain repeatedly, had an interesting trip through the Dolomites in 1881 and attended the International Medical Congress in Berlin in 1890. During all his vacations he spent much of his time in the open; he was devoted to fishing and was always interested in field botany. Many of his botanical trips were made with his brother Arthur, who later became an authority on the Myxomycetes. During the latter half of his life, Lister became an ardent follower of Audubon, and could frequently be seen, when on his vacations, with a vasculum over one shoulder and a bird glass over the other.

One of the most dramatic occasions of this period of his life was the Pasteur Jubilee celebrated at the Sorbonne on December 27, 1892. After the opening speeches by the president of the republic and the dean of the section, Lister was called upon

as the representative of the Royal Society of London and the Royal Society of Edinburgh. His address is a delightfully enthusiastic appreciation of the work of the great Frenchman. At the close of the speech,

> Monsieur Pasteur se lève pour embrasser M. Lister. L'étreinte de ces deux hommes était comme la representation vivante de la fraternité de la science dans le soulagement de l'humanité.

During the usual spring holiday in the next year, 1893, Lord and Lady Lister (for he had now been made a peer of the realm) went to the Italian Riviera. After about a week of botanizing, Lady Lister suddenly developed an acute pneumonia which resulted fatally.

Lister was now a solitary man. They had had no children and he and his wife had been intimate sharers of their mutual joys and sorrows for thirty-seven years. She had always entered keenly into his scientific work and the numerous "commonplace books" in which are recorded his many experimental observations are largely in her handwriting.

The year before his wife's death Lister had been retired at the age of sixty-five from the professorship at King's College, but his duties at the hospital continued through 1893. At the end of that session he ceased his work in the wards and also terminated most of his private practise. He was still mentally and physically vigorous but the death of his wife and the cessation of his regular duties depressed him very greatly.

Declining Years, 1893–1912

With the death of his wife social gatherings at Park Crescent came largely to an end and he seemed to have no zest even for his experimental work. To give him some regular occupation his friends obtained for him the foreign secretaryship of the Royal Society in November, 1893. He held this position until 1895, when he was elected president. His life for the next four or five years consisted of his duties in this society, officiating at many public ceremonies, and serving in the upper house of Parliament. A number of interesting general addresses date from this period of his life.

In the autumn of 1897 Lister again visited America on the occasion of the annual meetings of the British Association for the Advancement of Science at Toronto and the British Medical Association at Montreal. Following the public ceremonies, Lister and his companions made a tour of the west, taking in Vancouver, Seattle and Yellowstone Park.

At the second Tuberculosis Congress in 1901, Lister presided on the occasion of Koch's paper relative to the non-infectiousness of the bovine tubercle bacillus for human beings. Lister's impromptu criticism of this paper showed a remarkable grasp of the subject and gave proof of the fact that his mind was still keen in analysis in that he pointed out several of the weak points in Koch's argument.

On the occasion of his eightieth birthday, his professional friends determined to gather all of his more important papers and addresses into a memorial volume. He was very much interested in this publication and took an active part in aiding the committee who had it in charge. The collected papers were finally published in June, 1919, in two quarto volumes with many reproductions of Lister's original drawings.

The last seven or eight years of Lister's life were tedious and certainly years of "labor and sorrow." In the latter part of 1909 his sight and hearing became much impaired and although he continued to dictate letters, he could neither read nor write. His sister-in-law, Miss Syme, was with him continually during the latter years of his life and ministered to his comfort. Pandora's gift was also still with him and to the very last he hoped to be able to finish an unpublished research on the nature and cause of suppuration. He died February 12, 1912. By his own wish he was not buried in Westminster Abbey, but at the West Hampstead Cemetery beside his wife.

It is not difficult at this period to evaluate the work of Joseph Lister. With all due credit to Semmelweiss and Lemaire, it was the driving force of Lister and his work that played a large part in the overthrow of the reign of "laudable pus" and the crowning of the far-reaching experiments of Pasteur with the effective methods of modern surgery. Aside from this, his greatest work, he will be remembered for his painstaking observations on the contractile tissues of the iris, and his overthrow of the then current theory that coagulation of the blood was due to the liberation of ammonia, showing instead that in the blood vessels at least it depended upon injury to the tissues. His paper on excision of the wrist for caries is a classic, and the introduction of the catgut suture in surgery of the vascular system, and absorbent gauze bandages in all open wounds were definite advance steps.

Lister in all his work was diligent and painstaking, not spectacular or brilliant. As an operator he was usually slow, punctilious in the arrest of hemorrhage and in the stitching of wounds. His hands were steady and skilful. He planned each

operation before beginning it, laying out his instruments as he mentally rehearsed the operation step by step. His voice was musical and in public he spoke deliberately and clearly. He had a slight stammer which was scarcely noticeable when he felt fresh and vigorous. From the first he was much respected by his students and as years passed this developed into an enthusiastic devotion. John Stewart writes in 1910, "The difficulty will be for any man to find language to express what our Master was to us. We knew we were in contact with a genius, we felt we were helping in the making of history and that all things were becoming new." Lister was almost worshiped by his patients. He was so extremely sympathetic, so gracious in his manner, and so attentive to all their wants. His thoughtful face showed obvious mastery of himself and of his situation.

The general feeling towards Lister both now and during the later years of his life is well expressed by a happy phrase of Sir Michael Foster's at the end of a speech delivered at Toronto,

> In early life Lister belonged to a Society the members of which called all men Friends, and now in turn because of his inestimable beneficence and service to mankind, all men the world over call him Friend.

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FOOD TASTES AND FOOD PREJUDICES OF MEN AND DOGS

By VILHJALMUR STEFANSSON

NEW YORK CITY

DURING eleven years spent within the Arctic circle, I have made various observations on the food prejudices of dogs which appear to me to be of interest not only in themselves but also in the light they throw on the food prejudices (or tastes) of human beings.

When we were on our way down the Mackenzie River on our second expedition, we purchased some dogs. From my knowledge of the country, I take it for certain that the diet of these dogs during their bringing up had been as follows: fresh, putrid, and wind or smoke-dried fish, with caribou, moose and possibly mountain sheep meat in the same three states of preservation. There would also be a certain amount of rabbits, ptarmigans and possibly some fresh-water ducks. We can be quite certain that eider ducks and seals were both absent from their dietary. When we got this team down to the Arctic coast we found that all of them at first refused to eat seal meat if the seal meat was fresh, but would eat it readily if it was semi-putrid. The refusal to eat fresh meat was so steadfast that some of the dogs starved themselves for at least a week before tasting any of it.

The following spring we had some of the dogs from this team at the mouth of the Colville River in Alaska, and it happened that the only food we could conveniently secure for them was eider ducks. They all at first refused to eat the eider ducks and again some of them went through a starvation period of more than a week before even nibbling at the meat. But they were all willing to eat eider ducks as soon as they became putrid.

In the spring of 1912 I found myself on the north coast of Alaska east of Point Barrow, with a team of dogs some of which had been purchased from the Eskimos of Coronation Gulf. These dogs had been brought up on a diet of seal and caribou meat with possibly a little fish in summer. It is reasonably certain that they had never had opportunity to taste waterfowl of any kind. On the Alaska coast we were now unable for a period to secure anything except geese, chiefly American white-fronted and white wavies. For several days all of the

dogs in the team refused to eat and one dog persisted for more than a week before eating at all, although he had to work part of the time. These geese were fresh.

In 1910 we had a female dog brought up on the Booth Islands, near Cape Parry. She had had for food nothing but seal and polar bear meat during her first year of life. We then moved inland to where the only available food was caribou. It was more than a week before she would taste caribou meat.

In 1914 we had in northwest Banks Island a team of dogs none of which presumably had ever eaten wolf meat. It is very inconvenient to have in a team animals that will not eat any food that happens to be available, so we make a practice of breaking all our dogs of all their food prejudices when we can. We now happened to kill a wolf and, although we had plenty of other meat, we took the occasion to break our team to the eating of wolf meat, thinking that some time we might find it convenient to be able to feed them wolf. We did not know exactly the ages of our dogs, but could judge them roughly by the teeth. One of the dogs was presumably two or three years older than any other member of the team. There were six dogs altogether. We offered them the meat for three or four days before any of them ate any of it. Then they began to eat it, and they commenced to eat it in the order of their age, the youngest being the first to give in. The oldest dog went for two weeks without swallowing any of the wolf meat, although he occasionally took a piece of it in his mouth and dropped it again. He had been fat at the beginning of the experiment and had become skin-poor when at the end of the two weeks we had to commence travelling and were forced to feed him on caribou meat, for we needed his strength in the work we were doing. We never had the time to break this dog to the eating of wolf meat. It is possible that actual starvation would never have done this with the fresh meat, but he could have been taught to eat it by feeding him putrid wolf meat first and then later on pieces of wolf meat that were fresher and fresher. We did induce him to swallow some small pieces of wolf meat by dipping them in rancid seal oil. Obviously he did not smell or taste anything but the seal oil and took these pieces to be seal meat.

This is only a synopsis of our experiments with the food tastes of dogs, from which I have drawn the following generalized conclusions:

Dogs brought up around ships and used to foraging in slop pails and eating highly seasoned food will eat any food that we have ever offered to them. It seems, therefore, that a dog used to many sorts does not mind eating one sort more.

Dogs brought up on a diet restricted to two or three articles will, if they are more than a year old, always refuse at first when an entirely new food is offered to them. They base this refusal on the sense of smell and if the meat is putrid enough so that the putrefaction smell completely hides the native smell of the meat then the dog has no objection. In other words, all rotten meats taste and smell substantially alike and are, therefore, recognized as a familiar diet, while any new kind of fresh meat offends the dog through its strange smell.

It is commonly said by hunters and natives who have noticed that dogs will not eat wolf or fox meat that dogs object to cannibalism. I find that the objection of a dog to wolf meat is no stronger than his objection to duck meat or caribou meat, provided that the duck or caribou is an absolutely new meat in the experience of the dog.

I have found that the food prejudice is stronger the older the dog, and I incline to the belief, although we have not a sufficiently large number of experiments to be certain, that with dogs of a given age the prejudice of the female against new food is stronger than that of the male. This seems to strengthen the position of those who believe that women are more conservative than men by showing that conservatism as a sex character is fundamental and extends down into the lower animals.

I have thought that it would be exceedingly interesting to make further experiments in the food tastes of dogs along the following lines:

Newborn pups of the same litter should be selected, one to be fed for two years on mutton and water, another on fish and water, a third on beef, and a fourth perhaps on a vegetarian diet. It would make the experiment more interesting if a male and a female could be used for each sort of diet. Judging from our experiments, it seems probable that at the end of two years the mutton-fed dog would refuse both beef and fish, and the fish-fed dog would refuse both mutton and beef. I believe it would also be found that the abhorrence for the new diet would be stronger with the female in each pair than with the male.

It is well known that some Eskimo groups eat either no vegetable food at all or else practically none. But in all parts where we have been except in Coronation Gulf they are fond of the berry known in Alaska as the "salmon berry" (called in Labrador: Baked apple). We were, therefore, astonished, especially my Alaska Eskimo companions, when we found that the Coronation Gulf Eskimos live among an abundance of these berries and have never thought of tasting them. On inquiry we found there was no taboo against the berries and my Eskimo

companions tried to introduce the fashion. They found no difficulty in getting children to try the berries, except that in some cases the mothers were offended and considered it quite improper for my companions to try to induce children to eat this unheard-of food. The grown men were commonly willing to taste the berries and I do not recall that even one man refused, but I should say that fully half the women positively refused even to taste the salmon berry during the summer we spent with them. This is really a rather good fruit and I have no doubt that by now most or all of the people are eating the salmon berry, but our observation that first year indicated clearly the greater conservatism of the women. We observed this conservatism in many other things—for instance, smoking. Although all western Eskimo women use tobacco and although there have been tobacco-using women on our ships when we have come in contact with these eastern Eskimos, we have always found the men far readier than the women to learn smoking.

I looked into the matter of whether the reluctance of the women to smoke, or to eat salmon berries was due to the idea that things proper for men might be improper for women. It did not seem that any such consideration applied.

I have had a great deal to do with the food prejudices of white men in connection with introducing them to a diet of meat only. I have found that the laws of food prejudice as deduced from dogs have applied to my white companions exactly. Other things being equal, the older the man the more he has objected to trying a new kind of food and to abandoning the foods he was used to. We found with dogs that one brought up on a ship and used to a great variety in diet would take readily to a new diet. Similarly we have found that "well-brought up" men, used in their homes to a large variety of foods, both domestic and imported, take very readily to any new thing (such, for instance, as seal meat). But men "poorly brought-up" and used only to half a dozen or so articles of food in their regular diet, are generally very reluctant to try a new food unless it has been represented to them in advance as an expensive or specially delicious thing. Of course, the situation here is not so simple as it is with dogs. For one thing, the man of the laboring type has a feeling of being degraded when he is compelled to eat the food of "savages," while a man of the intellectual type is appealed to by the mild flavor of adventure in experimenting with "native" food.

THE SCIENTIFIC SELECTION OF MEN

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THE problem of selecting men for certain jobs and positions always has been one of vital importance both to the men and to the position. At the present time this problem is increasing in importance and is becoming difficult and complex because of differences of opinion in regard to the method and technique of making a selection of certain men for certain jobs, positions or types of work.

The new science of psychology has standardized certain scales, measures and tests for measuring general intelligence. The best known of these are the "Binet-Simon Scale for Measuring Intelligence." This scale has been revised, the revision being known as the "Stanford Revision of the Binet-Simon Scale."

The army psychologists during the recent war formulated and standardized the well-known "Alpha and Beta" tests for measuring the mental intelligence of large groups of men of varying degrees of education. It was found during the war that units made up of men of equal intelligence were much more effective than when men of unequal intelligence were placed in the same unit. It was also found in certain types of work and units such as artillery, signal corps, engineering corps, medical corps, etc., that a certain standard of intelligence was necessary before any individual could hope to be effective in that particular type of work. Our fighting army was rendered much more effective by this type of testing, selection and placement. There is no good reason why our working army in times of peace could not be made much more effective by the same methods.

Psychologists are now working upon scales and measures for testing the aptitudes of individuals for certain types of work. They know that people who possess certain special abilities are more successful on certain types of work than those that do not possess these certain abilities. They also know that people who possess certain disabilities can never hope to be successful on some particular types of work. This field is known as the "Psychology of Special Abilities and Disabilities."

The army psychologists also worked out tests for the measuring of trade knowledge and trade skill. These were very

effectively used during the war in choosing the men for the large proportion of technical positions now required in a modern army.

All of the above tests, scales and measures are based upon accurate scientific data. The tests have been furnished and standardized by using the same scientific methods that are used in any other branch of scientific research. There is no question about the validity of these tests and without doubt great advances will be made along these lines in the next few years.

The demand among industrial leaders for tests of these types to help solve the labor problem of industry has been so great during the last five years, that it has brought about a revival of the old abandoned so-called science of phrenology, physiognomy, character reading and character analysis. This revived pseudo-science has many followers among industrial executives, and the purpose of this article is to discover the validity of the technique and methods of this alleged science. If there is anything good in it, it most certainly should be recognized and made use of. If it is based upon fallacious assumptions, this fact also should be known.

Phrenology was founded and formulated before any of the recent startling discoveries of modern science were dreamed of. In 1815 Gall of Vienna coined the name phrenology and was its modern progenitor. In America the system was expounded by Caldwell, the Fowler Brothers and S. R. Wells; in England by Spurzheim, Combe and Elliotson. The scheme as founded by Gall is to inspect the scalp and facial features, and judge by them the possesion or non-possession of certain mental and moral characteristics. Each of these faculties is supposed to be located in a certain specified area of the brain. The entire number of these characteristics or faculties is given as forty-three, each with its own particular location. The high point of belief in this alleged science was reached in 1830, there being at that time thirty phrenological societies. At the present time the modern exponents of character analysis make use of phrenology, physiognomy, palmistry, and hand-writing.

The progress of the scientific study of the brain has rendered this entire school obsolete. Practical anatomy, physiology and applied psychology have proved beyond question that the exterior of the skull is by no means an index to mental processes, this article has been an earnest student of character analysis, characteristics, faculties, abilities or aptitudes. The writer of phrenology and physiognomy for many years, but is forced by the evidence to believe that the wide-sweeping claims of the so-called character analysts rests on assumptions entirely unwar-

ranted by the facts. Among certain groups of well-meaning but gullible people the plausible "demonstrations" of these so-called character analysts pass for scientific fact and unfortunately in some parts of the country employment managers and industrial executives have been taken in by the promise of the acquirement of omnipotence and infallibility in a few easy lessons for a fat fee paid to these "professors" of character analysis.

Character analysis is not scientific; it is misleading and without any foundation or scientific basis that justifies its use in any situation of importance. The following points will give the reasons for this stand against the use in industry of the so-called science of character analysis of the phrenological and physiognomical type.

1. Although cerebral localization for certain sensory and motor centers has been scientifically demonstrated, it has also been scientifically demonstrated that there is no similar cerebral localization for such general traits of character as self-esteem, acquisitiveness, secretiveness, constructiveness, combativeness, calculation, language, firmness, spirituality, number, time, tune, weight, color and the rest of the forty-three faculties claimed by the character analysts. This at once sweeps away the foundation upon which the entire body of claims of the character analysts rests. Their main stock in trade is the claim that certain protuberances or depressions in the skull and certain shapes and lines of the features of the face indicate the possession or non-possession of certain specified characteristics or abilities.

2. No scientific data have been presented to prove that there is any correspondence between size and functional capacity of any portion of the brain. No scientist has as yet established any correlation between definite areas of the brain and specific mental, moral or emotional qualities, but it is a scientifically established fact that there is no correlation between the volume, shape or weight of the brain and the general traits of character emphasized by the analysts.

3. The evidence presented leads us to believe that functional capacity depends on the complexity and type of structure, of chemical and molecular action, on the convolutions, fissures, and on the quality of the brain structure, rather than on mere mass, weight or shape.

4. It is a scientific fact that the shape and varying thickness of the bones of the head are no indication as to whether brain tissues of any kind or cerebro-spinal fluid will be found underneath. In other words, it is a scientifically established fact that the conformation of the brain does not necessarily follow the conformation of the outside of the skull.

5. The entire body of claims made by the character analysts is based upon limited and very casual observation of exceptional cases, the observational method being used almost entirely throughout. The method used throughout the development of this so-called science has been first to establish a hypothesis and then to search until some exceptional case is found that will fit this hypothesis. It is just as easy to find cases that do not fit as to find those that do.

6. Most of these claims for ability to analyze character are not based on objective evidence that can be scientifically measured, weighed, formulated and tested. In all fields of science research men submit their findings and their evidence to scientific tests by their colleagues and other scientists, before they give their findings to the world, or attempt to use them for financial gain.

7. On the basis of physical stigmata alone we are unable to decide the presence or absence of specific traits or abilities, nor are we able to decide whether any individual is a desperate or degenerate criminal, a pervert, a mediocre person or a genius, an idiot, an imbecile, a madman or a fool. Fine hair or delicate skin does not necessarily indicate delicacy of perception, judgment or sensitiveness. A "convex, concave, or plane" face is no criterion of the quality of the brain or of its functioning. Lombroso, the Italian criminologist, about fifty years ago formulated a system for the detection of criminals by physical stigmata, such as the shape of the forehead, the nostrils, the lips, chin, the width of certain portions of the head, etc., but before his death his entire theory was exploded by Gross, the German psychologist, and was abandoned by Lombroso himself.

8. Up to the present time no character analyst has presented any scientific data as proof for the wide-sweeping claims and assertions made by them.

9. On the basis of the above facts we feel justified in taking the stand that the claims of the so-called science of character analysis rest on assumptions entirely unwarranted by the facts. This being the case we can assume that its use in any situation of importance will be misleading and harmful.

One of the most fundamental phases of our present civilization and particularly of our industrial and commercial development is the application of science to all phases of our everyday life. The modern manufacturer and business man is daily becoming more and more of a scientist. He must, and is, developing the scientific attitude of mind. This is the attitude which the writer has taken toward character analysis and has resulted in the conclusions formulated above.

THE ART OF WRITING SCIENTIFIC REPORTS

By F. H. NORTON

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THE report is the medium by which the results of investigation and research are conveyed to those people who are, or can be, interested in the subject, and like all mediums, to be efficient, it should carry its message as quickly and as smoothly as possible. It should not only transmit the actual data obtained, the cold facts and figures, but it should also carry to the reader a feeling of confidence in the work accomplished, and should so coordinate the results with similar work, that their true significance can be realized. A well-written report forms a reasonable and coherent addition to the world's knowledge. As the purpose of the report is to transmit as smoothly and easily as possible, certain facts and ideas, to the average person likely to read it, it must, then, be written in a full and simple enough manner to be comprehended by the least tutored, and still not be boring to the most learned, of the group who may be considered as interested readers. This demands clearness of expression, a concise though complete treatment, and an interesting style of writing.

Perhaps the most important quality a report can have is interest, for if it is not interesting it will not be read and if it is not read, it has failed in its purpose. Of course, some discoveries are of such great importance that, even though imparted in the most unpalatable form, they are widely and eagerly read, but these are only occasional instances, for the average piece of research is not of enough value for many to take the trouble to read carefully a muddled and dry report of the work. On the other hand, many will read a clear and interesting report even on subjects to which they have before given little thought. It is easy to tell an interesting report from an uninteresting one, but it is more difficult to determine what features distinguish a good report from a poor one. The most important feature in the creation of interest is probably the style of writing. Style is a rather broad and indefinite term, and various readers will not agree on what constitutes a good style, but writing that leaves the reader's mind in such a condition that

it can uninterruptedly and contentedly follow the meaning of the report, without being conscious of the words, is what I should consider a good style. Like every other piece of writing the report should have a definite object. There should be no digression from it, and there should be no doubt in the reader's mind what that object is. Awkward and incoherent sentences, abrupt beginnings and endings of ideas, and incomplete expression all tend to interrupt the smooth flow of meaning from the paper to the mind. There are, however, certain cases, when, for the sake of emphasis, it is desirable to bring out a statement strongly, by an abrupt breaking of the sequence of ideas, but this method, to be of value should not be frequently used. Besides the rather obvious requisites for good style, there is that rather indefinite quality that expresses the individuality of the writer. Some writers have the happy faculty of making anything they touch upon interesting, and, although it is impossible to explain just why this should be, I believe that the author must be really interested in his subject before he can interest his readers.

To have a wide influence, a report must inspire confidence. When a statement is made, it must be based on something tangible, so that there will be no feeling of doubt or skepticism as to its derivation. I do not mean that no statement should be made unless it is an indisputable fact, for that would greatly, perhaps entirely, limit them, but enough information should be given on the methods and apparatus used in experimentation, to allow the reader to accurately judge of their validity. For example, no one would have any confidence in a curve plotted from experimental data if the actual points observed were not placed on the curve, or at least a discussion of their accuracy included. Before any serious experimental work is done, the precision of the observations should be determined, so that it can be definitely known just what confidence can be placed on the results. Perhaps the surest way of winning the confidence of the reader is to record the data from two runs taken under identical conditions, the check run serving to indicate what may be expected in the way of precision in the remaining data. When, as is often the case, certain conditions not under the control of the experimenter, as for example the weather, give a considerable degree of uncertainty to his results, it should be completely explained, for the least hint of a lack of frankness will defeat the purpose of that report and will leave any other report by the same writer open to suspicion.

It is, of course, unnecessary to emphasize the importance of absolute integrity in any scientific writing, and yet it is so easy for even the best intentioned experimenter to be unduly influenced by some preconceived idea, that it may not be out of place to take up this point. It is very easy in many reports, without deliberately altering the data obtained, to considerably change the results by omitting facts that do not substantiate the theory that is trying to be proved. There are cases when undoubtedly this is intentionally done, but on the whole the writer is not conscious of any dishonesty, but simply has evolved a certain theory, and can not conceive of any results being correct that do not fit in with that theory. It is the most difficult thing in the world to approach a subject with an open mind and draw conclusions with entire impartiality, in fact the human mind is not constituted that way, and only with practise and care can we approach that ideal condition. We are always trying to twist the facts, wherever they have the least flexibility, to fit together into a reasonable whole, and it takes the greatest care not to twist them beyond the permissible limits. It is always better to omit a whole set of data, part of which is in disagreement with the rest, than to assume one portion as correct, and find in further experiments, that the wrong portion had been published, an embarrassment that I am afraid a good many investigators have experienced.

In order that the reader may understand the report as quickly and with as little effort as possible, it should be written clearly. Clearness depends on several factors, the most important of which is to write exactly what is meant. It is very difficult for a person who has studied a certain subject thoroughly, to put himself in the reader's place, and write with enough completeness to cover all uncertain points. Facts which seem to him self evident, are unknown to the reader, so that it requires the greatest care to cover all uncertainties that may arise in his mind. It is often the case that a person can write more clearly on a subject which he is still struggling with, than after he has completely mastered it. On the other hand, it is equally injurious to clearness to write too much. Long introductions, lengthy descriptions and the carrying of deductions beyond the limit set by the completeness and accuracy of the data, all tend to obscure the meaning. One of the most common faults in reports is profuseness, some writers having developed the art of expressing a one sentence idea into a whole paragraph, to a high degree. Such spreading tires the reader so that he often does

not take the trouble to search a whole page to find the few sentences of value. Writing reports with the idea of covering as many pages as possible is an excellent way of limiting its readers to those whose time is of little value.

As in any kind of writing, clearness is dependent on the three essentials, unity, coherence and emphasis. A report should not cover too much territory or no one will take the time to search for the particular part they want, and any digression takes the mind away from the main thought. The writer should not rush into his subject at full speed neither should he take an unduly long time to get started, or the reader's interest will be lost, but he should lead up to his subject naturally and directly in a few sentences or a paragraph, stating the reasons for undertaking the report. It has been the practise with some writers to summarize briefly their report near the beginning. This enables one to tell at a glance just what the report is about, and except for very brief papers, should always be done. The material in the body of the report should be arranged in a logical manner, description of apparatus, methods of testing, results and conclusions. The end of a report remains longest in the reader's mind, so that there should be placed the statements which it is desired to give the most emphasis. It is good practise in long reports to place subtitles before each paragraph or group of paragraphs so that any particular portion can be found at a glance. After a report has been written, it often happens that the author has some further additions to make; and instead of rewriting the report, the additional material is usually added in the form of a supplement or appendix. This procedure somewhat destroys the literary quality of the report, but is sometimes necessary. It is, however, a great help to clearness to place in an appendix any mathematical proofs and computations that are not absolutely necessary for the understanding of the report.

Photographs and other illustrations are of the greatest value; first to make the report interesting, and second to make it clear and concise. A report well illustrated, especially with photographs will be much more widely read than one without illustrations. A picture creates interest, because it stimulates the imagination and because it shows under just what conditions the experimental work was conducted, giving a more personal note to the whole report. Further than this, a good illustration is often the quickest way to convey an idea, and every one likes to absorb his knowledge with the least effort.

It often happens that a good illustration will do the work of a whole page of description and in about one tenth of the reader's time. For the same reason results should be shown wherever possible in graphical form, a method that is almost universally used. When the results can be plotted as precisely as the data permit, there is no need of giving the data in tabular form. Each illustration should have a title and a short explanation, so that the reader can tell at once what he is looking at without the necessity of searching through the text, and it is also advisable to place the illustrations as closely as possible to the portion of the text to which they refer.

There are many writers, some good and some poor, who convey an air of self-importance in their works which does much to detract from their popularity. It is right and natural that a man should be proud of a good piece of work that he has accomplished, but to constantly remind the reader of it, is certainly poor taste. A good report may be safely allowed to rest on its own merits, and will receive its recognition much quicker if accompanied by a degree of modesty. Again some writers make themselves unpopular by not giving credit in the report to the men who have been working under him during its preparation. No one ever gained anything in the long run by withholding proper credit, and I am sure they have never lost by showing the value of their assistants' services. Another bad habit some writers have is to depreciate the previous work of others along their line, and claim more originality for their work than the facts will permit. Such procedure leads only to unpopularity, and the public is never fooled for long. It is well, then, to write in a modest and straightforward manner, and to give others their due amount of credit.

There is always the problem of bringing together the so-called practical and theoretical men. The mathematician rather scorns the engineer who is not versed in higher mathematics, and the engineer, in turn, often regards the mathematician's work as a waste of white paper. It is strange how distinctly separated are the two classes and how few men really sympathize with both. It can be more or less truly said that the engineer's definition of theoretical, is something he can not understand, and the mathematician's definition of practical is something he can not do. As the mathematician is chiefly known through his writings, it is important that he present his ideas in a form that will do the greatest amount of good; that is, his reports should be read not only by his fellow mathe-

maticians, but also by the men who can make use of them in engineering and industrial development.

Even though a man has the training and the ability to handle higher mathematics, unless he has the time to frequently use it, it will soon become difficult for him to follow carefully the work of others. Also the engineer is too busy to spend much time on theory, even if he were able to handle it easily. There are, of course, a few men who are practical engineers and at the same time great mathematicians, but such men are few, as the average engineer feels that he can better obtain a mathematician to do his work for him than to spend the time himself.

For these reasons, the average scientific reports should not, as is too often the case, be so filled with complex mathematical discussions that real results and conclusions are so completely obscured that the engineer can not take the time to find them. In order to bring the physical meaning out clearly, plots should be frequently used. For example a complex equation representing harmonic motion would mean very little to most people, but a curve of the motion against time would show at a glance just what was meant.

There are a great many reports whose influence would be tremendously increased by the reduction of the number of integral signs and an increase in the number of illustrations. Most people will naturally skip quickly over any mathematics that occurs in a report in the same way that they would dodge a patch of mud in the street, and if the mud becomes too thick they will take some other way, and yet these are the people who could make the greatest use of the material contained in the report. On the other hand every one will look at a picture, especially a photograph, even though they are only remotely interested in the subject, and often their curosity will be aroused enough so that they will completely read the report. It is much easier to write a description than to make a drawing or take a photograph, but when it is considered that each person reading the report will be saved several minutes by doing the latter, there is no question of its value. In the same way it is easier to write an abstract equation than it is to give a clear physical conception of the relation, but the latter will save most readers much time and trouble.

There are of course reports which consist primarily of pure mathematics, and are of use chiefly to the mathematician, so that the writer can assume that all of his readers will have such an understanding of allied work, that it will be unnecessary to

go into detailed discussion of his results. And there are works of such advanced and original thought that it is impossible to describe them in terms that would be understood by any except the most illustrious scientists. Such works as these are unfortunately in a vast minority, and there are very few scientific reports that have no material of direct value to the engineer and the industrial chemist or physicist.

It may be well to say a few words about the mechanical side of report writing, and in doing so it will be necessary to digress a bit to the methods of experimentation and collection of data. All data should be collected logically and neatly, and everything should be labelled, as what may be clear and obvious at one time may be quite obscure after a lapse of a few months. Again it is well to work up the data as far as possible as the work goes on, not only to check up on the validity of the observations, and to determine the completeness of the data required, but also to get the results in such shape that they can later be readily combined into a report, and no pains should be spared to constantly check up the results obtained in order to detect errors that might affect a long series of experiments. A complete photographic record should be made as the work progresses, or it may be necessary later to go to considerable extra work to obtain an illustration.

After all the necessary data have been collected, it is generally best to construct an outline so that the methods used, the results and the conclusions may be placed in logical order and, after this, all plots and other illustrations should be prepared and numbered in their proper sequence. The first draft of the report can then be written or dictated, and a dictaphone is a very convenient instrument for this purpose. Very few can write a report just as they wish it the first time, and like all other kinds of careful composition it requires a considerable amount of revision. It is of great value to have the aid and criticism of others in this stage, as the author loses his sense of perspective after working for a long time on one report.

In conclusion, the purpose of the report should be to carry some fact or theory so interestingly, so briefly, and so clearly that the busy world will stop to read it, and having read it will pause to think, for the ability to make men think in a new way should be the aim of every writer.

THE ESSENTIAL CHARACTERISTICS OF UNITED STATES CLIMATES[1]

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Climatic Provinces of the United States.—For purposes of general description, it is convenient to subdivide the United States into certain climatic districts or provinces. The largest, or *Eastern*, extends from the eastern margin of the Great Plains, which roughly coincides with the 20 inch annual rainfall line and also with the 100th meridian, to the Atlantic Ocean, and southward nearly to the Gulf of Mexico. The strip bordering on the Gulf may be set apart as a subordinate district, the *Gulf* province. The *Plains* province includes the Great Plains proper, and extends westward to the Rocky Mountains. Between the Rocky Mountains and the Sierra Nevada-Cascade ranges comes the *Plateau* province. The Pacific Slope constitutes a natural climatic region which may be called the *Pacific* province.[2]

The differences between north and south, resulting from differences in latitude, suggest a further subdivision of the Plains, Plateau, and Pacific provinces into northern and southern sections. Similarly, the Gulf province occupies the more southern latitudes of the Eastern province.

The Eastern Climatic Province.—The Eastern Province, enormous as is its extent, is nevertheless characterized by great uniformity in its climatic conditions and in its weather types. It has a continental climate, but with abundant, or at any rate sufficient rainfall. Over most of it the seasons are strongly contrasted. The summers are very warm and the winters cold. But the hot summers, with sufficient rainfall, usually insure abundant harvests, and the cold winters, while severe in northern sections, are on the whole stimulating and tonic. The influence of the Atlantic Ocean is minimized by the fact that the prevailing winds are off-shore throughout the year, being north-

[1] Reference may be made to the following papers by the same author: "Two Climatic Cross-Sections of the United States," *Mo. Wea. Rev.*, Vol. 40, 1912, pp. 1909–1917; "Lorin Blodget's 'Climatology of the United States.' An Appreciation," *ibid.*, Vol. 42, 1914, pp. 23–27.

[2] See R. DeC. Ward, "Climatic Subdivisions of the United States," *Bull. Amer. Geogr. Soc.*, Vol. 47, 1915, pp. 672–680.

west in winter over much of the coast, and southwest in summer. Thus it follows that there can be but little of the tempering effect usually associated with conservative ocean waters, and the coastal belt, except when the wind blows onshore under general cyclonic or anticyclonic controls, or when, in summer, local sea breezes occur, does not differ very much from the interior. One aspect of this situation is clearly illustrated on the chart of equal annual ranges of temperature. The large ranges of the interior are carried eastward to the coast, and even on to the ocean for some distance offshore. The continentality of the Atlantic coast climate, with its slight marine modifications, was well described by Blodget when he wrote that on the immediate coast "a local oceanic climate exists, but it is always blended with the continental features which belong to this part of the continent generally."[3]

There are only relatively slight and unimportant differences of topography. The whole area is freely open, to Canada on the north; to the Atlantic Ocean on the east; to the Gulf of Mexico on the south. With the warm Gulf on the south and the cold Canadian plains on the north, the winter temperature-gradients between north and south are unusually steep. In January, the isotherms over the eastern United States are very closely crowded together. The temperature then decreases northward at the rate of 2.7° Fahr. in each degree of latitude, both on the Atlantic coast and in the Mississippi Valley. This is an extraordinarily rapid temperature-gradient, and may be contrasted with the very much weaker gradient along the western coast of Europe in winter. As seen on the chart of mean annual temperatures, one may go north in western Europe a distance of a thousand miles without finding a change of temperature as great as that met with in half as long a journey along the eastern coast of the United States. In summer, North America is well and relatively very uniformly warmed. There is then much less difference of temperature between south and north. The temperature-gradient is greatly weakened. It becomes 1.1° along the eastern coast, and 0.7° in the Mississippi Valley. The temperature conditions may be very briefly generalized as follows:

District	Mean Annual	Jan.	July	Abs. Max.	Abs. Min.
N	40°	5°–10°	65°	100°–105°	—40° to —50°
S	65°–70°	50°–55°+	80°+	105°	zero–10°

The average dates of first and last frost, broadly generalized, are as follows:

[3] Lorin Blodget, "Climatology of the United States," 1857.

District	Last Spring	First Autumn	Av. Length of Growing Season
N	After June 1 (extreme N.)	Sept. (extreme N.)	3–4 months
S	Before March 1	November	7 months and over

Besides having a great uniformity of temperature conditions, considering its extent, the eastern climatic district of the United States also has, as a whole, a plentiful rainfall, well distributed throughout the year. Disregarding local areas on the mountains, the annual rainfall is greatest (50 + inches) towards the Gulf, and on the South Atlantic coast, most of the supply of water vapor for this precipitation coming from the Gulf and Atlantic Ocean, and decreases from about 40–45 inches over much of the north and central Atlantic coast and Ohio valley to 30–40 inches over the prairies, and 20 inches at about the 100th meridian. Although the rainfall is comparatively small at the western margin of the district, it is very well apportioned through the year. The maximum over the great farming states of the Mississippi valley region comes when it is most needed, *i.e.*, in the growing season of late spring and early summer. Of this region it has been well said:

> Although droughts sometimes affect considerable districts, and floods occasionally devastate the larger valleys, yet the world hardly contains so large an area as this so well adapted to civilized occupation.[4]

Nowhere in this district is there permanent necessity of irrigation, as there is in many places farther west. The rainfall comes chiefly from the ordinary cyclonic storms of the prevailing westerly winds. The spring and early summer rainfall of the Mississippi valley and adjacent regions is largely a local thundershower rain, and naturally occurs with the greatest frequency during the warmer months. In the colder season, with high pressures and with winds prevailingly blowing out from the continent, there is less opportunity for precipitation. In late summer the rainfall along the Gulf and Atlantic coasts is generally at a maximum, for the inflowing winds are then very warm and moist, and there are many thunderstorms. Along these coasts occasional West Indian hurricanes, in August, September, or October, give heavy rains and not infrequently damage buildings and crops. In Florida, the heavy August and September rains are almost tropical in character. In Tennessee and adjacent parts of Mississippi, North Carolina, Georgia, and Alabama there is a late winter or early spring maximum of precipitation.

[4] W. M. Davis, "Elementary Meteorology," page 301.

Rain falls most often, and hence the number of rainy days and the rain probability are greatest, in the Great Lakes region. This same district is also more cloudy than any other portion of the country except the northwest coast. The explanation of these conditions is found in the frequency of cyclonic storms, accompanied by general rains and by extended cloud sheets, over the Great Lakes and along the St. Lawrence Valley. Snow falls throughout a long season and in considerable amounts over these same northern and northeastern sections. Towards the south, the snow season becomes shorter and shorter; the snow lies on the ground less and less time. It finally becomes an almost, and then an entirely negligible factor.

Rapid and marked weather changes are characteristic of the eastern United States. These "paroxysms of change" (Blodget), which occur during the passage of numerous well-developed cyclonic storms, are unique for their suddenness, frequency and amount, especially in winter. They result from the bringing together, around the passing low-pressure centers, of winds of different directions and of very different temperature and moisture conditions, depending upon the characteristics of the regions from which these winds come. The winter dry northwest winds are often piercingly cold, coming as they do directly from the cold continental interior, and may cause a fall of temperature of 30° to 50° Fahr. in twenty-four hours, reducing the temperature to zero or lower ("cold waves"). They blow on the rear of cyclonic storms, and follow warmer and damper southerly or easterly winds which bring rain or snow. Several short periods of extreme cold are to be looked for every winter. During the warmer months, similar northwest winds bring the summer cool spells. The warmest weather, in winter as in summer, is brought by southerly and southwesterly winds which, coming from lower and warmer latitudes, blow north into a passing storm-center. In winter these southerly spells, while "unseasonably" warm, serve to break the monotony of the cold. In summer, with its high temperatures and high relative humidity, this same "sirocco" weather type is uncomfortably muggy and depressing, and often brings sunstrokes. The summers always bring spells of extreme heat of this type. In winter, the northeast cyclonic winds on the northern Atlantic coast are damp and unpleasantly chilly, blowing onshore from the cold ocean waters. In summer, their low temperatures bring welcome relief from the heat. In such a region, where sudden and irregular weather changes are so characteristic, climatic averages can give little

idea of the actual conditions which are experienced from day to day. Seasons often differ markedly in character from year to year. Weather changes are erratic and unexpected. It has become a popular saying that almost any kind of weather may be expected at any time of the year.

Thunderstorms, many of them of wide extent and of great violence, are characteristic summer phenomena over the eastern and southern United States. The far more severe tornadoes, fortunately of much less frequent occurrence, develop most often over the great lowlands of the central and upper Mississippi and of the lower Missouri Valleys. Tornadoes, as well as the more violent thunderstorms, have a habit of springing up along the dividing ("wind-shift") line between the warm southerly ("sirocco") type of weather in front of a passing summer cyclonic storm and the cool northwesterly (summer cool wave) type on its rear.

The Gulf Province.—Over the southern tier of States bordering on the Gulf of Mexico the temperatures are higher; the winters are much milder; the summers are longer and hotter; the rainfall is heavier, and has a late summer or early autumn maximum. With increasing distance from the most frequented cyclonic paths, which follow along the northern border of the United States, the cyclonic control is weaker; the irregular wind, temperature and weather changes are fewer, less sudden, and less emphatic; the diurnal phenomena are more marked, even in winter; the weather is more stable; conditions are more settled. In winter, cyclones which give heavy snows and marked wind changes followed by cold waves over northern sections, often bring but little cloud and rain, without marked temperature variations, over the Gulf States. The southern States are, nevertheless, subject to occasional invasions of considerable cold in winter, during the prevalence of north and northwest winds on the rear of well-developed cyclonic storms. These winter cold waves sweep east and southeast as far as the southern Atlantic and Gulf coasts, but their intensity diminishes rapidly as they advance into more southern latitudes. The occurrence in districts of high mean annual temperature of frosts of sufficient severity to injure the more sensitive crops, is a characteristic of southeastern North America, and is one of the marked, and economically one of the most unfortunate, features of the climate of the Gulf Province, which is otherwise in many ways singularly favored. Snow becomes a rarity. Over the sections immediately adjacent to the Gulf, it is practically negligible. On the warm, damp lowlands there is the

wealth of southern cotton, and sugar cane, and semi-tropical fruits, and from the truck gardens of the southern Atlantic coast immense quantities of early vegetables are shipped north, by rail and by water, to the markets of the great northern cities. The severe winters in the north effectually put a stop to most outdoor occupations during the colder months, although giving rise to others, such, *e.g.*, as ice-cutting and lumbering. In the Gulf Province, on the other hand, and over the closely adjoining sections of the Eastern Province, most agricultural and other outdoor work can be continued throughout the year.

The Plains Province.—The essential difference between the climate of the Great Plains and that of the Eastern province is not so much one of general temperature conditions as of rainfall. The Plains, like the rest of the great region lying to the eastward, have a continental climate. They lie at a distance from large bodies of water, and have massive mountain barriers on the west, between them and the Pacific Ocean.

The following table summarizes the temperatures:

District	Mean Annual	Jan.	July	Abs. Max.	Abs. Min.
N	40°	0°–10°	65°–70°	105°–110°	—50° to —60°
S	65°	40°–50°	80°–85°	110°	zero

As compared with the eastern States, the Plains have larger diurnal ranges of temperature; more abundant sunshine; drier air; greater evaporation; smaller rain probability; less rain; more wind. The prevailing winds have a monsoonal character: they are northerly and northwesterly in winter and southerly and southeasterly in summer. There is a well-marked diurnal variation in velocity on days whose weather is not under strong cyclonic control.

Rainfall is the fundamental climatic factor. From the more abundant precipitation towards the Atlantic and Gulf, the mean annual rainfall decreases westward until, at the eastern margin of the Plains, it averages about 20 inches, and towards the western margin decreases still further to below 15 inches. No sudden change, either in topography or in climate, takes place along the 100th meridian, but where the mean annual rainfall averages less than 20 inches, the amount is too small, under ordinary circumstances, for permanently successful agriculture, on a large scale, without irrigation. Another disadvantage of a small rainfall is its variability from year to year. Remarkable results have been attained by "dry farming" methods over parts of the Plains, in the case of certain crops which require little water, and in seasons with favorable rain-

fall. Dry farming is, however, a precarious venture, and can not be counted on to give results comparable with those obtained in a well-watered or well-irrigated country. The natural limitations of the country are clearly recognized. Its best use is not to be found in the old-time boundless cattle ranges, nor in vast farms which depend solely upon the natural rainfall, but rather in smaller individual farms and cattle ranches, where irrigation from streams or from ground-water is possible. The relatively high and steady velocities of the winds over the Plains have proved a reliable source of power in driving the windmills which are widely used for pumping water for irrigation. This relation of the 20 inch rainfall line to agriculture has locally long been recognized. "East of it lies success; west of it, failure. Look out for the 'dead-line.'" The critical boundary thus termed the "dead-line" has played an important part in the settlement and development of the Great Plains.

The distribution of the rainfall in normal years is singularly favorable over most of the Plains province. The maximum usually comes in late spring or early summer, when moisture is most needed by the growing crops. To the southward (New Mexico), the maximum is retarded until mid- or late summer. These warm-season rains are spasmodic, and fall chiefly in the form of brief and local thundershowers. Winter is the dry season. In the north, the winter precipitation is mostly in the form of snow, but the amounts are considerably less than those of the same latitudes farther east. Over the southern Plains, where the winters are warmer, the winter snows average under 10 inches in depth, and are even under 5 inches in western Texas and southernmost New Mexico.

Cyclonic control is less marked than in the east, partly because fewer cyclonic storms pass over or near the Plains, and partly because the storms are generally of a milder type. Irregular weather changes are most frequent over the northern Plains States, which are nearer the main storm track. In the south, the sequence of weather changes is much more uniform. Diurnal rather than cyclonic controls are dominant most of the year. The more severe winter cyclones bring sudden temperature changes, severe gales, driving snow and extreme cold ("blizzards"). Cold waves, sweeping southward and eastward from western Canada, occasionally reach as far south as Texas, where they cause sudden and marked falls in temperature, with chilling northerly winds ("northers"). Much of the winter weather, however, is dry, clear, settled and bracing. Locally, along the eastern base of the Rocky Mountains, the

winter cold is often tempered by warm *chinook* winds. Long spells of hot, dry, typical, diurnal weather, with southerly winds, are characteristic of much of the summer. When continued for many days, or sometimes even for weeks, unbroken by general rains or by widespread thundershowers, such spells are associated with extended droughts and result in injury to crops, especially when accompanied by certain hot and dry winds which are characteristic of the Great Plains ("hot winds"). While thunderstorms are of frequent occurrence during the warmer months, tornadoes are relatively rare.

On the west, the Plains province gradually merges into the eastern foothills and slopes of the Rocky Mountains. Here the greater elevations and the topographic irregularities give rise to special climatic features which are associated with mountain and plateau climates. Colorado has become famous for its health resorts. With a small annual rainfall; light winter precipitation; few storms; little cloudiness; dry, stimulating air, and comparative immunity from many of the sudden and severe weather changes characteristic of the east, there are evident advantages for invalids in this region.

The Plateau Province.—The *Plateau* province is a great interior region of very diversified topography. It has a wide range of mountain, high plateau and arid lowland climates, superposed upon and causing local modifications of the general dry continental climate of the province as a whole. "Climatological topography" (Blodget) is here highly significant. The diversity of topography results in a very "patchy" distribution of climates and of vegetation, as local variations of temperature, frost, rainfall, etc., may determine. The winds, also, are largely topographically controlled, both in the case of the local mountain and valley breezes and of the more general cyclonic wind directions.

The outstanding characteristic is the small rainfall, which, however, shows marked increase with altitude. The higher mountains and plateaus have distinctly more precipitation than the neighboring lowlands. With the exception of local areas in the mountains, the mean annual rainfall is everywhere less than 20 inches; it is mostly below 10 inches, and over no insignificant portion of the southwest it is even below 5 inches. This is the region of what, not many decades ago, was known as the "Great American Desert"; of Great Salt Lake; of the interior drainage basis of Nevada, the "artificial State." The real "American desert" is in southeastern California, southwestern Arizona and western Nevada. Death Valley is here, with

its famous borax and its intense summer heat. The Salton Sea is here—an anomaly in a true desert. The Black Rock desert; the "sinks," and the soda deposits of western Nevada are here. With the high mountain barrier on the west, the whole plateau district is in the rainshadow. Arid or semi-arid conditions are to be expected, except where the higher mountains or plateaus give rise to more plentiful precipitation, especially in the north, where general storms occur more frequently, and where the barrier is less effective. Even in the south, where the rainfall is less, the higher elevations are fairly well watered. The name of the Aquarius Plateau is a suggestive one. The Rocky Mountains are so far from the Pacific that their rainfall is not as a whole heavy. They are, furthermore, to leeward of the considerable ranges of the Sierra Nevada-Cascades.

Irrigation is made necessary by reason of the deficient rainfall, although dry-farming is carried on in certain sections, as, *e.g.*, in eastern Washington, with considerable success. Wherever the streams, supplied by the melting snows and the heavier rainfall of the higher mountains, afford sufficient water, there green oases of varied crops, dotted with fruit and shade trees, break the monotony of the "desert." Most of the winter precipitation is in the form of snow, which is decidedly heavier in the north than in the south, and over the higher elevations than over the lower lands. In the far southwest, snow is rarely seen except on the mountains. The rainfall distribution through the year varies greatly in different portions of the district. In the north, there is generally a late winter or early spring maximum. In the south, the primary maximum usually comes in late summer, with a secondary maximum in winter. The winter rains and snows are cyclonic. Frequent, often daily, thunderstorm rains are characteristic of the warmer months, especially over the mountains. The rain probability is below 20 per cent., and the minimum number of rainy days in the United States is found in the far southwest (under 30).

Temperatures are so largely controlled by the topography that even broad generalization is almost impossible. The essential facts may roughly be given as follows:

District	Mean Annual	Jan.	July	Abs. Max.	Abs. Min.
N	50°	25°–30°	65°–70°	100°–105°	—10° to —30°
S	60°–70°	40°–50°+	80°–90°+	110°–120°+	zero to 20°

Minimum temperatures and frost occurrence are mostly a matter of local topographic control and of local air drainage.

The dates of first and last frost are very variable. The summers of the southern lowlands are long and intensely hot, but the low humidity is an important factor in making the high temperatures endurable. The southern winters are comparatively mild, dry and bracing. In the north, the heat of the summer is much less severe, and the winters are colder. The districts west of the Rocky Mountains are, however, to a great extent protected against severe cold waves of the eastern type. Dry, stimulating air; an abundance of sunshine; large diurnal ranges of temperature—these are dominant characteristics of the Plateau climates, taken in the large. Diurnal ranges of 40° or more are by no means uncommon. Cool nights follow hot days in summer, especially on the mountains and plateaus. Periodic diurnal, rather than irregular cyclonic, weather types are dominant.

The Plateau province is beyond the reach of most of the cyclonic storms which so largely control the weather and climate of the rest of the country. At intervals, winter storms, coming from the Pacific, cross the northern portion of the district on their eastward course along the northern track, and other, but not greatly frequented, storm tracks cross the central and southern parts. Hence, as a whole, there is a decided lack of sudden, irregular and severe weather changes. The maximum cyclonic control, here as elsewhere, is in the colder months, but few of the winter storms bring heavy precipitation. Even in winter, long spells of fine, bright weather, with light winds, moderately warm days and cold nights are common. Summer weather is characteristically fine and settled, broken by afternoon or evening thundershowers, and occasionally by the cloud sheet and general rains of a passing summer cyclonic storm.

The Pacific Province.—Over the narrow Pacific coastal belt climatic conditions are quite unlike those elsewhere in the country, and in many respects resemble those of northwestern and western Europe, including the Mediterranean area. Blodget gave an excellent brief yet comprehensive climatic comparison when he wrote that "the Pacific coast climates are Norwegian, English and Spanish or Portuguese, with the intermediate France blotted out."[5] The similarity of climates in southern California and in the countries bordering upon the Mediterranean explains the similarity of many of the agricultural products and fruits, and of the general methods of cultivation, in these two regions. Exposed to the influence of the warm

[5] Lorin Blodget, *loc. cit.*

Pacific, with the prevailing westerly winds coming directly from the conservative ocean, and protected on the east by high mountains, the Slope as a whole has a modified marine or windward coast climate. A typical west coast subtropical climate is found in California. The wide range of latitude between north and south, together with the varying topographic controls and the differences of exposure to the ocean influences, explain the great variety of climates which are found in this province. These range from those of the rainy and densely forested slopes of Washington to those of semi-arid southern California; from those of the lowlands to those of the snow-covered mountain tops; from the cool summers of the coast to the hot summers of the Great Valley. The climate is, in general, mild and equable, with slight diurnal and seasonal ranges. The relatively small seasonal change is common both to the cool and humid climate of the far northwest and to the warm and dry climate of the southern interior.

The following table summarizes, in a very general way, the essential temperature characteristics of the Pacific province.

District	Mean Annual	Jan.	July	Abs. Max.	Abs. Min.
N	50°–55°	35°–40°	60°–65°+	95°–105°	10°–0°
S	65°±	50°–55°	65°–75°+	110°–115°	20°±–10°

The relatively high winter temperatures insure the Pacific coast harbors against freezing, whereas on the northern Atlantic coast ice not infrequently causes difficulty to navigation in severe winters. Further, the mountain barrier of the great Sierra Nevada and Cascade ranges to a large extent keeps out the extremes of winter cold which are found over the interior districts to the east. Under the influence of the warm ocean current eddy which circulates from right to left in the Bay of Alaska, and of the cool return current which flows along California and Mexico on its way equatorward, the isotherms along the Pacific coast are spread far apart. Hence there are but slight differences of temperature between north and south along the coast, and the rates of temperature-decrease per latitude degree from San Diego to Sitka are but 0.95° in January, 0.65° in July, and 0.8° for the year. These temperature-gradients may be compared with the much steeper gradients of the Eastern province, previously referred to. The chart of equal annual ranges of temperature shows clearly how small is the seasonal change of temperature along this coastal strip (not over 25°), while across the mountains, in the interior, the ranges reach 40°, 50° and even 60°. No such severe winter

cold waves are experienced here as to the east of the Rocky Mountains, or even over the Plateau. The area over which frost does not occur annually includes southern California. In the region around San Diego, killing frost occurs in less than half of the winters. The attractions of outdoor life during the colder months in southern California have made this locality a favorite winter resort for those who wish to escape the rigors of more severe northern latitudes.

The interior valley of California, between the Coast Range and the Sierra Nevada, being well shut off from the ocean, is very hot, dry and sunny in summer, especially in the south, while the immediate sea-coast is cool, damp and often foggy. The crowded July isotherms show very clearly the difference between the temperatures of the interior and those of the coast. This is one of the most remarkable contrasts in the world over so restricted an area. The strong diurnal ranges of temperature over the interior valley give relatively cool nights and the dryness of the air relieves the great heat of the days. The high inland summer temperatures result in the prevalence of cool onshore daytime winds, which are unusually well developed at San Francisco. Sea-breezes are a characteristic feature of the whole coast.

The rainfall is heavy (over 100 inches) on the northwestern coast of Washington, and decreases rapidly to the south, to about 10 inches on the extreme southern coast of California, and less than 10 inches in the San Joaquin Valley. There is a marked seasonal periodicity. The maximum comes in winter. General cyclonic storms are then most frequent; they then travel farther south; the land is colder than the ocean; the prevailing winds have more of a southerly component. Some southern districts are actually without any rain in summer, and then become very dry and dusty. In the north, however, it rains more or less all through the summer months, although the winter maximum remains. The rainfall migrates from north to south as winter approaches, *i.e.*, the rains in the south are distinctly subtropical in character, and correspond to the winter rains of the Mediterranean districts of Europe. As Woeikof first pointed out, these rains extend farther north in western North America than happens anywhere else in the world.[6] The rains come when the "stormy westerly winds" are farthest south, and cease when the northward migration of the tropical high pressure belt displaces the storm-bearing westerlies polewards. Even in the so-called "rainy season"

[6] A. Woeikof, "Die Klimate der Erde," 1887, p. 24.

of winter, the rains are generally light; they are not steady and continuous, usually lasting but two or three days and separated by spells of fine, sunny weather. On the extreme northwest coast, rain falls on nearly half of the days in the year. There is a marked decrease in the number of rainy days, as well as in the mean annual rainfall, from north to south. From the point of view of an outdoor life, southern California is favored in having very few rainy days. Over the lowlands of the Pacific Slope, snowfall is of little importance. Even in the north it is very light, and on the coast it becomes practically a negligible factor south of the northern boundary of California. On the mountains, however, even in southern California, snow falls frequently, and on the western slopes of the high Sierras and of the Cascades it is very heavy. The water-supply from these melting mountain snowfields is of supreme importance in irrigating the fertile and productive valleys and lowlands, whose natural water supply, in the form of rainfall, is insufficient for agriculture and for fruit-raising.

The Pacific Slope is not subjected to violent weather changes. During the winter, a series of general cyclonic storms moves eastward across the northern portion of the province, and occasional storms come in farther south, or their paths carry them south to the more southern latitudes. Hence the northern districts have more rain, more cloud, and more frequent weather changes than the southern. The amount and distribution of the annual rainfall are controlled by the number, intensity and paths of the winter storms. When these extend farther southward, and are better developed, the rainfall is heavier and more widely distributed. In general, rain falls with the cyclonic winds in the southern quadrants, and fair weather prevails with northerly cyclonic winds. The changes in wind direction bring changes of temperature, but the general temperature gradient being weak, and the regions from which the different winds come not differing greatly in temperature, sudden and marked rises and falls of the thermometer do not occur. In general, southerly to westerly winds are warm in winter and cool in summer; northerly to easterly winds are cool in winter and warm in summer. The rains of summer, where they occur, are as a rule light and local in character, and fall mostly in the north and on the mountains. The cold-season precipitation comes in the form of general cyclonic rainy spells. Thunderstorms are infrequent, and usually light; rarer on the coast and more common inland and on the higher mountains. Special cyclonic winds, having certain marked

peculiarities, occur over some sections, under the combined control of the pressure distribution and of the topography. The "northers" of the great valley of California have a chinook or foehn character. They are dry and dusty and may be injurious to crops. The "Santa Ana" of southern California is also a hot, dry and dusty wind, somewhat similar to the norther. It blows from northerly or easterly points, and is trying to men and animals. Coming from the dry interior, it reaches the southern coast with the characteristics of a desert wind.

The Valley of California is a great agricultural and fruit-raising district, as is the Willamette Valley in Oregon. The summer dry season is a favorable climatic feature during harvest-time, and for drying fruits in the open air. The western slopes of the Sierra Nevada are well watered and forested, while the eastern (leeward) slopes are dry. On the Cascades and on the northern Coast Range there are also abundant forests. North of San Francisco, on the western slope of the Coast Range, are the famous redwood trees, and the "Oregon pine," from farther north, has been known the world over because of its usefulness for ships' masts and spars.

BIBLIOGRAPHIC NOTE.—For general statistical information concerning all parts of the United States reference may be made to the "Summaries of Climatological Data by Sections" (*Bulletin W*, U. S. Weather Bureau, 4to, Washington, D. C., 1912. 2 vols. Price 5 cents for each section; 10 cents for any three sections; $3 for entire set unbound. To be obtained from the Superintendent of Documents, Washington, D. C.) The period covered by the data varies. Reprints are issued from time to time for the various sections, containing all the data included in the first edition and also bringing the observations down to later dates. Several important additions to the tables have also been made in the reprints. (See also, R. DeC. Ward, "A Short Bibliography of United States Climatology," *Journ. Geogr.*, Vol. 17, Dec., 1918, pp. 137–144.)

THE PROGRESS OF SCIENCE

THE SCIENTIFIC MEETINGS AT CHICAGO

THE American Association for the Advancement of Science and the special scientific societies affiliated with it meet at Chicago during the week beginning December 27, this being the seventy-third meeting of the association, the eighteenth convocation week meeting of scientific societies, and the third of the greater convocation week meetings held at four year intervals in Washington, New York and Chicago. It is planned that at these meetings in our large centers of scientific population there shall be a full representation of scientific societies and special efforts to promote science and the appreciation of science by the largest possible public.

The promise for the Chicago meeting is auspicious. In the past four years science has successfully met the supreme emergency of war and in the ensuing reconstruction period it will be the principal factor in our complicated civilization. These facts are perhaps more nearly recognized than ever before, and the approaching meeting will do much both to advance science and to secure increased support of science by public recognition. It is also the case that every four year period signalizes a further development of scientific institutions and scientific activities in the central and western states, so that the center of scientific population bids fare to correspond with the general center of population, and this circumstance will be reflected by the approaching meeting.

Dr. L. O. Howard, chief of the Bureau of Entomology, United States Department of Agriculture, the president for the Chicago meeting, has been permanent secretary of the association for twenty-two years, during which time the membership has increased from 1,729 to over 12,000 and the meetings and other activities of the association have grown in proportion. As the Chicago meeting celebrates the completion of Dr. Howard's successful administration, so it marks the beginning of the secretaryship of Dr. Burton E. Livingston, professor of plant physiology at the Johns Hopkins University, during whose term of office it may be expected that the association will progress even more rapidly than the science of the country on which it depends and which it in turn promotes.

The address of the retiring president will be given at the opening general session on the evening of December 27 by Dr. Simon Flexner, director of the Rockefeller Institute for Medical Research. Sessions of general interest are planned for fourteen sections of the association, representing different fields of science, and the retiring vice-presidents for the sections will each give an address on some broader aspect of his own field. Some forty affiliated and associated societies will hold meetings for the reading of papers and for discussion.

Most of the sessions will be held at the University of Chicago which in a comparatively short period has taken its place among the three or four universities which are most active in the advancement of science. Only two previous meetings have been held in Chicago, the first in 1866 and the second in 1908. The

earlier of these meetings represented a new era in the work of the association, the meetings of which had been suspended during the war. There had been an informal meeting at Buffalo in 1864, attended by 79 persons, and a meeting the following year at Burlington was attended by 73. At the time of the meeting in 1868 the membership of the association was 428 and there were 259 in attendance. The sessions were held in the assembly hall of the Y. M. C. A. and the Baptist church, the meeting being held under the auspices of the citizens rather than of any educational or scientific body.

The meeting of 1908 was held at the University of Chicago with an attendance of 723 members, and it was estimated that, counting members of the affiliated societies, there were some 2,000 scientific men in attendance. Dr. William H. Welch, of the Johns Hopkins University, gave the presidential address; Professor Edward L. Nichols, of Cornell University, presided, and the president elected was Professor T. C. Chamberlin, of the University of Chicago. It was one of the most notable meetings of the association that has been held, and it may be expected that the meeting to be held at the end of December will make new advances in scientific organization, in scientific research and in the appreciation and support of science by the general public.

PROBLEMS OF A NATIONAL ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

AN interchange of delegates between the American and British Associations has been planned, and a joint meeting of the two associations on the common ground of Canada could be arranged with advantage both to science and to international friendship. The British Association, founded in 1831, was a model for ours, and we still have much to learn from it. For example, it is doubtful whether the addresses of the president and vice-president of the American Association always maintain as high an average standard of style, proportion and general interest as those presented at Cardiff, parts of which have been printed in this journal.

There may also be faults that the British and American Assocation share and, in so far as this is the case, the vigorous criticism of the conduct of the British Association that has been printed in *Nature* and other English journals since the Cardiff meeting should be of interest here as well as there. Indeed it must be admitted that the failure of the association to keep in touch with the general public—which is the principal charge against the British Association—is even more obvious in the United States. There is, however, this difference that while the English press and the people still pay more attention to the scientific work of the association and treat with greater consideration men engaged in research, there is a tendency for the two countries to reach a common level. Formerly the London *Times* gave whole pages daily for a week to the British Association, while now the space is reduced to a column or two and the topics treated are likely to be education, economics and the like. It seems that the same decrease in interest, especially in the more serious aspects of scientific work, is shown at the place of meeting. Still, it is the case that the local committee at Cardiff spent about $7,500 in entertaining the association, which the Chicago committee would probably regard as beyond the resources of that rich city.

Some of the comments regardng

the British Association are as follows:

I think that our "British Association" is in an unhealthy condition owing to the attempt made by it—not deliberately, but by constitutional looseness of purpose—to combine the features of a friendly picnic and smoking debate with the work of a national conference dealing (under the disadvantage of public ignorance and journalistic inaccuracy) with great questions of national importance. A choice must be made between "picnic" and "conference." I should prefer the picnic. —E. RAY LANKESTER.

Unless the British Association becomes democratic and acts as a real bond of union between scientific men and the thinking public, rather than as a periodic platform for personages, it does not seem to fulfil any function worth continuing. The public application of science is a totally different thing from applied science. This scientific synthesis and the direction of the unique mental attitude, induced only by the actual discovery of new knowledge, to the conduct of public affairs are the real and peculiar functions of the association if it is to regain its national position. Curved space, isotopes, and the economics of life on the floor of the ocean are topics of great interest to hundreds of the public. The standards of truth which science has set up, and the elevation of its pursuit above sophistry, chicanery, and the monotonous motives of self-interest, inspire the imagination of hundreds of thousands. The British Association seems to be attacked by senile paralysis just as a belief in science and in the power of its methods is arising in the world phœnix-like from the ashes of its old self.—FREDERICK SODDY.

Your criticism of the British Association, that it fails to touch our national life, is most opportune; but whereas you imply merely that it is decadent, to me it seems to be practically defunct. An active worker on its behalf in the past, I have little hope of its resuscitation and doubt if it can ever again fulfil the desires of its early promoters, who undoubtedly held its primary function to be that of advancing public appreciation of scientific discovery. I have always deplored our failure to appeal to the public. Seemingly, the spirit of sacrifice is gone out of science; strange to say, the herd instinct is altogether wanting in our society, so uncontrolled is our individuality. The assumed author of "The Beggar's Opera," after remarking of his characters, "There's not an honourable man among them, nor an honest woman," proceeds to say, "but they are all human." So are the present exploiters of the British Association, though were it not human to be selfish some might even dub them inhuman on account of the narrowness of their outlook and their disregard of public needs.—HENRY E. ARMSTRONG.

These are the more extreme criticisms. Letters and articles in defense of the association have also appeared and the honorary secretaries, Professors J. L. Myers and H. H. Turner have prepared a statement answering criticisms and outlining plans for the future work of the association.

THE ORGANIZATION OF SCIENTIFIC RESEARCH UNDER THE BRITISH GOVERNMENT

OUR federal government has more scientific men in its bureaus and devotes larger sums to scientific work than any other nation. An immense amount of useful work is accomplished, but individual initiative is likely to be submerged by routine. It may, therefore, be worth while to note how the British Government has met the situation during and since the war period.

The last report of the Committee of the Privy Council for Scientific and Industrial Research contains an account by Sir William McCormick of its activities since its foundation in 1915. According to an abstract in the *Spectator* the program which has been elaborated by the advisory council during these five years classes its functions under four main heads. First comes the encouragement of individual workers at research. The special claims made by the needs of the war practically denuded the universities of research workers. Now that peace has returned other dangers have arisen. On the one hand, the universities are overcrowded with students, and the demands of teaching

are leading to an impending shortage of those research workers, without whom no science can advance. On the other hand, the revival of industry is tempting men away from "the corps of research workers" into the service of the great manufacturing corporations. During the past four academical years the Advisory Council has spent about £50,000 in maintenance grants to research workers, and it expects to spend nearly as much in the forthcoming year. "No conditions are attached to the grants made to workers whose sole aim is the extension of knowledge, either as to the line of their work or as to the use to be made of the results," though, of course, great care is taken that the recipients of state aid should be genuine workers at the solution of a knotty problem.

The second part of the program includes the organization of research associations for special industries. A large and wealthy firm has its own laboratories and its own staff of chemists or physicists. But in many industries the firms are too small to do this work for themselves, and in all industries cooperation saves waste and duplication of effort. A fund of a million sterling was authorized by Parliament to assist in the establishment of research associations, of which eighteen are already at work and five others have been approved. The firms connected with the existing associations have undertaken to raise an annual sum of nearly £40,000 for their purposes, and it is probable that this income will be largely exceeded as the benefits derived from research become practically apparent. The industries which already have research associations in existence are those of cotton, linen, wool, leather, bootmaking, laundering, sugar, cocoa and jam, motoring, glass, iron, non-ferrous metals, rubber, photography, scientific instruments, Portland cement, refractory materials, and shale oil. It is much to be hoped that the rapid growth of these promising infants will soon convince industry as a whole of the necessity for scientific research as a basis for the profitable union of Capital and Labor.

The third item in the program of the Advisory Council is the conduct and co-ordination of national research; in other words, provision for dealing at the national expense "with certain fundamental problems which were of such wide application that no single industry, however intelligent or highly organized, could hope to grapple with them effectively." The first of these basic problems is that of fuel, to study which the Fuel Research Board was established in 1917. Closely allied is the conservation of coal and mineral resources. The problem of the preservation of food and that of building materials and construction are also being handled by special Boards. The Geological Survey and the National Physical Laboratory have been handed over to the Research Department. The Radio Research Board takes its place beside three new boards, dealing respectively with chemistry, physics and engineering, to coordinate the scientific work still required for the fighting services. The fourth item of the program is the granting of aid to scientific and professional societies for purposes of research.

ENGLISH MEDICAL LABORATORIES

MEDICAL service in England is more elaborately organized under the control of the state than in the United States. There is a Ministry of Health which has appointed a medical consultive council and this council has presented a report which

is now under consideration. The *British Medical Journal* prints an abstract according to which it is pointed out that medical treatment must be both individual and communal, and that individual treatment may be either domiciliary or institutional. By communal treatment is to be understood pre-natal and maternity care, child welfare, the inspection and treatment of school children, dental treatment, clinics for early tuberculosis, and facilities for physical culture. The general conception is that domiciliary treatment in each district should be based on a "Primary Health Center." This institution would be conducted by the general practitioners of the district, and would be provided with an efficient nursing service. It would be linked with a "Secondary Health Center" in some town conveniently placed in relation to the means of communication. Opportunities would be provided for consultation, either at the primary center or at the homes of the patients, with physicians, surgeons, and specialists of the secondary center, which would itself be brought into corresponding relation with a medical school hospital.

A sketch is given of a scheme for laboratory services. The first and essential function of the laboratory at a primary center would be to give facilities to the general practitioner; it would be so equipped as to enable him personally to make any examinations he desired to undertake. The equipment would be supplied from the secondary center and adapted to meet any increase in knowledge, skill and interest displayed by the practitioners concerned. Some person must be placed in charge of such a laboratory; it is thought that his qualifications might vary with the size, geographical situation, and special needs of the center.

The laboratory of a secondary center should provide ample accommodation and equipment, and its senior staff should be highly qualified to do clinical laboratory work, both for the primary center and for the hospital of the secondary center; it would also do postgraduate teaching. The head of the laboratory would be a well qualified pathologist, and the staff would comprise specialists in morbid anatomy, bacteriology, and pathological chemistry. Some of the assistants would be preparing for the higher posts in the service; others in an intermediate class, would furnish recruits for the staff of the primary center.

The laboratory of the secondary center would be linked with the chief or university center, which would have a special laboratory for health services distinct from a professorial department, though in close contact with it. A separate health services laboratory would protect the academic staff from routine, leaving it free for teaching and fundamental research work. The staff of the health services laboratory would be recruited from that service and be dependent upon meritorious performance within it. At the same time it is recognized that no rigid barrier should be erected between the academic pathologist and those engaged in the health services. Though there would be two distinct careers, circumstances would often cause individuals to pass from one to the other.

The health services laboratory of a university center in addition to the routine work, would carry out the more difficult investigations referred to it from the subsidiary centers, but it might also undertake much of the routine work for the university hospital. The director of such a laboratory would rank with a university professor, but would usually be a distinguished

pathologist with tastes inclined to organization and administration rather than teaching.

With regard to research, it is suggested that the organization throughout the proposed health service would facilitate inquiry into the causes of disease. It is suggested that the facts showing the need for inquiry might often be brought together in the first place by the medical practitioners in a locality; the manner in which it should be further prosecuted would depend upon the subject, but research into fundamental problems would still continue to be conducted through the university and the Medical Research Council. A national organization of laboratories in touch with every branch of the health services would, it is believed, provide opportunity for systematic investigation and team work. The director of the health services laboratory at the university center would be in a position to start machinery for the intensve investigation of any urgent problem, and with the goodwill of the general practitioners the investigation would include cases never available in the wards of a hospital. In particular, the earliest stages of disease might be made the subject of organized research, which, it is hoped, might be subsidized and perhaps supervised by the Medical Research Council.

SCIENTIFIC ITEMS

WE record with regret the death of Harmon Northrop Morse, for forty-four years associate and professor of chemistry at the Johns Hopkins University; of Samuel James Meltzer, head of the department of physiology and pharmacology of the Rockefeller Institute for Medical Research; of Arthur Searle, professor emeritus of astronomy at Harvard University; of Isadore Dyer, dean of the medical school of Tulane University; of Yves Delage, professor of zoology at the Sorbonne, Paris, and of Sven Leonhard Törnquist, professor of geology at Lund.

NOBEL prizes have been awarded to Dr. Jules Bordet, professor of bacteriology at Brussels, and Dr. August Krogh, professor of physiology at Copenhagen.—Professor Stephen Moulton Babcock, of the University of Wisconsin, inventor of the Babcock test for determining the amount of butter fat in milk, reached his seventy-seventh birthday on October 22, and in honor of the event and of his work a university convocation was held. Professor Babcock is engaged in active work in his laboratory.

THE International Congress of Mathematicians at the Strasbourg meeting accepted the invitation presented by Professor Leonard E. Dickson to hold the next congress in New York in 1924.—At the International Congress of Physiologists, held in Paris, it was resolved, on the invitation of Sir E. Sharpey Schafer, to hold the next meeting in Edinburgh in 1923.

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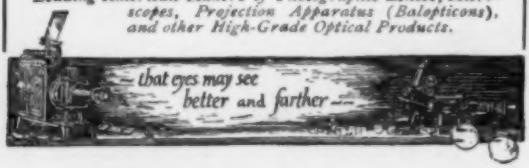

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